$7.00 per annum
Single numbers $2.00

QUARTERLY

January, 1930
Volume III, Number 1

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

JANUARY, 1930

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

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THE GEOTROPIC ORIENTATION OF GASTROPODS*[1]

From the Laboratory of General Physiology, Harvard University

W. J. Crozier and A. E. Navez

I

The geotropic orientation of a creeping gastropod such as *Helix* and similar forms presents for examination a number of questions which have not always been kept distinct in discussions of geotropism. We have been concerned to learn something about geotropic orientation as a function of the circumstances which may govern its expression. Only in this way can there be obtained such formulations of geotropic excitation as may make it possible to investigate behavior in situations such that gravitational as well as other (for example photic) stimulations are also at work. A review of certain experiments directed to this end has been given in another place (16). For our present purpose it is necessary to regard separately such points as the following: (1) the latent period for geotropic response, under proper and standardized conditions; (2) the speed of orientation; (3) the *position* of stable orientation during progression; (4) the speed of oriented progression. Each of these must be studied as a function of the exciting component of gravity.

A reasonably complete understanding of the case requires also a theory of the relations of the innervations and central nervous regulations of tensions in the parietal musculature, upon the one hand, and, on the other, the release and the locomotor efficiency of the pedal waves. It was pointed out some years ago that the orientation of gastropods permits one in these forms to separate clearly the parts respectively played by asymmetric muscle tensions involved in producing orientation and by the locomotor organs in effecting progression (3, 20). Creeping is here due to the activity of the foot in producing pedal waves; with a certain few exceptions (11, 62) the

*Received in the Editorial Office, March 28, 1929.

[1]Based in part upon observations collected at the Harvard Station, Soledad, Cuba (Atkins Foundation), and in the vicinity of Cienfuegos Bay. We are under obligation to Mr. W. E. Leonard and his associates for much kindness during our stay at the Tropical Station.

body musculature plays no part in creeping; while the foot, so far as concerns its production of pedal waves, must be regarded as an "independent effector" secondarily under (largely inhibitive) central nervous control, since the laws of its activity are the same regardless of the load transported by its movements. The latter statement is based upon experiments with *Limax* (28, 24), upon the results of successful attempts to dissociate *creeping* and *orientation* (22, 23, 25), and upon subsequent work in this laboratory with *Limax* and with *Helix* not as yet published; it is in agreement, also, with the outcome of attempts to isolate the foot as a creeping organ completely cut off from the cerebral ganglia, or at least isolated from it [a brief review of citations is given by Crozier and Pilz (28)]. A reservation must be made with regard to this matter of the primary responsibility for turning (orientation) as independent of the foot and its activity in locomotion. There arise exceptional cases in *Chiton* (60, 2), in *Cyprea* (58), and, as we have recently observed, in *Acanthochiton,* the direction in which pedal waves course over the foot may be modified in such a way as to determine the direction of turning. In *Chiton* this modification is slight, and is assisted by the partial twisting of the foot through the contraction of the lateral body-muscles. In *Cyprea* the effect is much more pronounced, and it is possible that in the case of arhythmic pedal organs [(59, 11) ; and cases studied more recently, which we will describe elsewhere] the effect may be of a somewhat similar kind. More interesting for our present purpose is the state of affairs in several species of *Chondropoma.* These progress by means of alternate *ditaxic* direct waves. During steady creeping, one wave at a time is visible on the surface of the foot, one beginning on one side when the preceding wave on the other side is completed. In turning to one side, the foot is contracted on that side, which serves as a sort of pivot, locomotor waves appearing only on the outer, relaxed half. As is the rule, however, general contraction of the foot of a snail inhibits or prevents the appearance of pedal waves, so that the result described is one which appears to follow directly from the fact that the contraction of the longitudinal elements of the pedal mass is bilaterally controlled in an independent way. Nevertheless, there are relations, presumably involving properties of central nervous structures, between the release of the pedal mechanism so that it actively forms pedal waves effective in progression and

the expression of orientation. In the case of phototropic movements (20, 24) the amplitude of orientation under certain conditions may be limited by the speed of creeping, while under other conditions the limitation is of a reciprocal sort, the whole relationship suggesting a central partition of impulses flowing, on the one hand, to the pedal mechanism and, on the other hand, to the parietal musculature responsible for the execution of the orienting posture. When one turns to geotropic orientation, the connection is somewhat different. Only during active progression does real geotropic orientation take place. By geotropic orientation we mean the bending of the head to one side through increase of tension in the contractile elements of that side, in a manner definitely related to the pull of gravity. This may mean either that creeping movements and postural movement are released together, as a result of appropriate excitation, or that, only when the pedal mechanism is in operation can the usually efficient stimulus achieve postural expression. At the moment we are not called upon to decide this point, but merely note its relevance to the subsequent discussion.

It has, in the first place, a very definite bearing upon attempts to express the *speed* and the *amount* of orientation as a function of the exciting force.

II

Reports by Delage (41) and Loeb (53) first drew attention to the problems presented by the nature and the mechanism of oriented behavior of animals in the earth's gravitational field. Loeb (54) emphasized the fact of unequal contractile tensions on the opposite symmetrical sides of an organism as leading to orientation, and referred to the possibility that *equal* tensions on the two sides might result in locomotion parallel to "lines of force" in the gravitational field; herein, at the time, lay his justification of the term "geotropism." Subsequently, the theoretical foundation for interpretations of this sort has grown enormously, and the original significance of the term has somewhat altered. Two general modes of interpretation have been proposed for geotropic orientations in animals. It has been supposed that a statolith organ (statocyst) may be in general concerned with the regulation of oriented movement in the earth's gravitational field, or on a turntable. With reference to gastropods, this view has appealed, for example, to Baunacke (5)

and Kanda (48). As Cole (7) has very properly pointed out, the experimental basis for the opinion has been far from convincing, and it has involved a certain confusion between the presumptive or probable rôle of the "statocyst" or other organ functionally significant as such, as a *positional* organ (7, 76, 62, 65) and as a controller of oriented progression. On the other hand, the pull of the body mass as exerted upon the supporting musculature, or upon the stretched integument of distorted regions of the body, has been appealed to as a source of proprioceptive excitations. On this latter view, which is inviting in the case of animals not possessing a statocyst, such as *Chiton* and in certain other cases (2), upward or downward geotropic orientation might be expected to cease with the assumption of such a position (on a sloping surface) that the downward pull of the mass of the body was (within a threshold difference) equally borne by the two sides of the body (6). Moreover, on a surface of given slope, orientation should be accelerated, made more definite, by increasing the mass carried by the supporting structures. This is indeed the fact with such forms as *Limax, Helix,* and others. These questions cannot be approached by determining percentages of a given assemblage of individuals which react at all, or which have reacted after some fixed time (16). What is needed is information which will connect the amount of the exciting force with some measurable feature of the response as given by those individuals which do exhibit the behavior one seeks to analyze. In the absence of such data only remote indications can be gotten from the experiments of Davenport and Perkins (39), for example, or from those of Kanda (48); these tend to show, however, that there does exist, in gastropods, a connection of some sort between the slope of the surface and the speed and precision of the exhibition of geotropism (30).

Cole (7) found that there is a definite relation, in *Helix,* between the slope of á surface and the speed with which upward ascent is made. At this time it had not been recognized (29, 30) that the angle of ascent upon a sloping surface is also a function of the slope, so that the method of measurement used by Cole was defective to the extent that deviation from a path directly upward becomes greater and greater the lower the slope; this has been discussed previously (30). Cole gave an approximately rectilinear relationship bebetween $\sin \alpha$, where α is the slope of the surface and hence $\sin$

α a proportionate measure of the gravity-component parallel to the surface, and the speed of progression; the actual figures can be understood if both speed of progression [as in rats (72)] *and* extent of upward turning are nearly proportional to *log sin* α. Cole pointed to the difficulty of regarding this aspect of geotropic response as being controlled by the otolith and stressed the likelihood that the effect was entirely similar to the orientations evident upon a turntable, or produced by pulling laterally upon the shell. The effect of attaching to the shell an additional mass, not too heavy, is to increase the speed of geotropically oriented progression on an inclined surface, although not when the snail is proceeding on the level (7), as well as to increase the speed and precision of the orientation itself (24).

To devise a scheme of excitation which will make the statocyst responsible for certain of these effects is, of course, not impossible. It might be supposed that the extent of the positional disturbance of the statolith determines the vigor of the responses, and likewise the effectiveness of tensions applied to peripheral regions. It is (or at least should be) perfectly evident that the theory of the proprioceptive control of geotropic orientation and progression need not exclude the functioning of the statocyst (when present) as a positional organ (16), and has indeed been specifically referred to in this connection (7, 30). But the results of a search for quantitative connections between gravitational excitation and response have made it rather futile to go very far in this direction (62, 35). Crozier and Pincus (29, 30) found that the *amount* of upward orientation of young rats (under carefully controlled conditions) upon an inclined surface was so related to the slope of the surface that the angle turned upward on the surface during steady progression was nearly a rectilinear function of *log sin* α. In this work and in subsequent repetitions of the experiments made for various purposes (30-35, 72, 27, 49) employing related material, the analysis has been controlled in a diversity of ways. The interpretation employed turns upon the assumption that the limitation of upward *turning,* during progression, is controlled by the establishment of a limiting position such that the downward pull of the body is (within a threshold difference) equally effective as a source of excitation on the two sides of the body. This analysis has furthermore been checked by the results of a considerable body of work with other forms [(16, 77, 35,

51, 36, 50, 45), and experiments with snails, larvae of the blowfly, beetles, and ants, flies, and others, not yet published]. One general result has been the proof that for forms in which the geometry of progression is different, the same empirical equations do not apply; for forms similar in this respect, the same empirical and the same rationally formulated equations do describe the extent of orientation as a function of the slope of the surface, and thus of the effective component of gravity. The effect of adding loads to the body mass is always to increase the amount of orientation; of decreasing the mass of the body (36), to decrease this amount. Another result (34), clearly forecast, in essentials, in the earlier work on this topic (30, 31), has been the devising of a method of treating the *variability* of the measured responses in terms of the same analysis. This phase of the matter we cannot now go into (cf. 33, 35), but it may be remarked that several writers who have undertaken to comment adversely on the interpretation provided have been very careful not to refer to it (47, 71, 46, cf. 34).

III

An experiment of this sort was undertaken by Wolf (77) with the slug *Agriolimax*. Wolf showed that the upward (or in geopositive phases, the downward) geotropic orientation during creeping on a glass surface was related to the slope of the surface, whether the slug is upon its upper or upon its under side, by the equation $(\Delta \sin \theta)\ (\Delta \cdot \sin \alpha) = -$ *const.*, where θ is the angle of orientation on the surface, α the angle of slope of the surface. As an approximation, $\Delta\ \theta / \Delta \log \sin \alpha$ is also (nearly) constant. Wolf showed that this could result from the adjustment to sensible equality of the pull of the animal's weight, in a direction parallel to the surface, upon the mutually inclined muscles (and integument) of the two sides of the anterior portion of the body.

It is obvious that if the control of the orientation originates in proprioceptive excitations, it should be modified predictably by changing the animal's weight. This is indeed the result with young rodents (30, 34) and with beetles (36), as with the geometrically different case of fiddler crabs (50). One method of introducing such modification, although not necessarily the simplest, nor in itself free from obvious objection, is to put the animals under water. The amount of orientation, on a surface of given slope, ought then to be

less. Experiments made by one of us several years ago with *Limax* showed this slug to be quite unsuitable for the purpose. The effect sought was indeed observed with *Lunatia,* by Pincus and Crozier, but not extensively enough under satisfactory conditions. In all probability the facts recorded by Walter (75) and by Dawson (40), namely, that with fresh-water snails geo-positive movements are seen, or geotropic indifference, while negative geotropism appeared only in those with no air in the lungs, are of similar import, though not unequivocally. Kanda (48) also observed more distinct geotropic responses of snails in air than when under water. Piéron (70), however, in a series of experiments with *Limax maximus,* claims to find that the geotropic response is just as definite under water as in air. He attempted to follow Wolf's procedure with *Agriolimax,* but apparently did not quite understand it. In fact, Piéron's competence to deal with such matters seems to be adequately revealed, on the theoretical side, by his sermon [also reproduced in the republished English translation of his paper, (71)] to those who would "oversimplify" the interpretation of behavior through the use of the tropism doctrine, on the ground that they neglect the activation of sense organs! [One may be pardoned for pointing out that this is a strange form of excitement to witness in a writer who, not so long ago (67; and related papers, 69) treated as phenomena of "memory" a group of effects connected with repetitive excitation by light, in snails—plainly (11) a matter of sensory adaptation; and who has also been quite anxious to attribute "memory" and analagous properties to phenomena of (to put it mildly) very dubious rhythmicity in anemones (68), when more careful scrutiny has shown unmistakably that sensory excitation of a direct sort is all that need be appealed to (cf. 61, 12).] The reply which is called for in the present connection is contained in the statement that in the case under discussion the "simplification" happens to be based upon a more extensive acquaintance with relevant facts, and upon less crude experimentation.

As regards *Limax* the facts are as follows: *Limax maximus* usually creeps so slowly, and, on an inclined surface, with such swervings of the path, that the angle we have termed θ is only feebly exhibited as a function of α. The theory thus far employed holds that within a zone, on the inclined surface, defined on either side by $+\theta$ and $-\theta$ orientation is not constrained. A long series of careful

observations with *Limax,* including many made some time ago under the direction of one of us by Miss Alice Beale and Miss Mary Sears, showed that it was in this respect an unsuitable object for such experiments, since with cessation of creeping, or much slowing of it, no definite orientation is exhibited. Taken as a whole, the percentage of more-or-less straight-upward trails increases as the slope of the surface is increased, which is due to the swerving already mentioned; but in general the trails are not straight enough for measurement. This holds when photic stimulation is excluded, either by removing the eye-tentacles or by working in a dark room under very dim red light, with a properly moistened surface.

When a *Limax,* attached to a glass plate, is put under water, in an aquarium or tub diffusely illuminated by the ordinary light of the laboratory, its subsequent movement is largely influenced by the manner of submersion. If the slug is placed with its axis horizontal, and the plate lowered edgewise into the water, the first movement is a turning of the head *away* from the side first brought into contact with the water. If the slug is head-down, its first movement is a turning to one side. The head may turn so far as to encounter the mid or posterior region of the body, and indeed usually does so. The head end may then attach to the substratum, loosely, and continue a progression movement along the margin of the posterior part of the body. It may extend farther, and so be regarded as creeping "straight up," although, unless some system of coordinate lines is marked on the plate, the judgment and recording of orientation-angles under water is a very deceptive affair; Piéron (70, 71) gives no details as to his procedure on this point. More interesting is the fact that the posterior end of the body rarely moves at all (experiments were made at various temperatures), while the attachment of the foot to the substratum (glass, wood, slate, have been tried), dependent upon an adhesive mucous, quickly becomes so feeble that the animal slides off to the bottom of the aquarium. A further source of difficulty in such tests is the fact (apparently ignored by Piéron) that, although the eye-tentacles are not fully everted under water, the eyes are sufficiently exposed to be stimulated by light, and partial orientation is witnessed away from windows or other sources of illumination. The greatest difficulty with *Limax,* however, is the fact that it simply does not creep under water. According to Piéron (70, 71), the animals he used oriented sharply, even

at very low slopes, under water (which we never find), and apparently crept well, though nothing definite is said as to this; as we have clearly indicated, active creeping is necessary for the assumption of θ as a function of the slope. His attempt to measure our orientation-angle θ was apparently unsuccessful. He attempts to use the deviation from "maximum slope" of the path of the slug in space, but his geometrical discussion leaves it completely mysterious as to what he actually measured. The position of the animal's axis in space may be of interest in certain formulations, but again it is easily shown that creeping is a prerequisite for orientation in *Limax*, and it has been perfectly clear in our experience that the degree of upward turning is correlated with the degree of extension of the body.

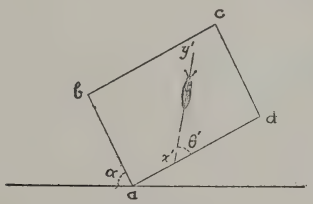

FIGURE 1

Copied from Piéron (71). See text. From this terminology, and the method described by Piéron for varying the inclination of the surface, apparently by tipping the glass plate on its corner, it is not clear just what the angle θ is supposed to measure.

Neither in the French text nor in the English version, however, are we able to obtain a clear idea of just what Piéron believed himself to be measuring. We reproduce in Figure 1 the diagram he gives, and a portion of his description of it. From this it appears that the fundamental purpose of such experiments as Wolf's has completely escaped him, because it is clear that the most elementary notions of geometry should enable one to avoid the error of supposing that the angle θ in Figure 1 is of any significance whatever. If *Limax maximus* in Europe does creep on glass under water the point would be worth re-examining, because with *Agriolimax* we have been able to obtain, with improved technic, lesser magnitudes of θ under water than in air, although the creeping is again slow and not very satisfactory—in some cases portions of the foot do not adhere, hence the posterior region of the body may be swung around mechanically. It is necessary to lower the plate with the attached slug horizontally into the water, then to tilt it.

Our interest in this matter led us to undertake a series of experiments with snails which might be expected to creep with ease both in air and under water. Favorable material was found in the abundant tree-snails of Cuba, which are pronouncedly geotropic, as well as in the intertidal pulmonate *Onchidium*. For assistance in the course of the work at Soledad and on the seashore near Cienfuegos Harbor, we are deeply indebted to our friend Mr. W. J. Clench, curator of Mollusks in the Museum of Comparative Zoölogy. We desired to obtain evidence of a sort which might be quite independent of measurements of amounts of orientation as bearing upon the possible rôle of the statocyst, which might give additional light upon the validity of the notion of proprioceptive, non-statocyst, control of geotropism.

IV

One might well expect in the case of snails found upon trees and bushes, or on vertical rock-surfaces, that negative orientation with respect to gravity might be found a prominent element in behavior. We have examined from this standpoint the conduct of large numbers of *Liguus fasciatus,* species or forms of *Cerion, Chondropoma, Pleurodonte auricoma, Cepolis,* two species of *Urocoptis, and Veronicella,* as well as of a species of *Onchidium*. For certain purposes the first mentioned is the most convenient.

Liguus fasciatus was found upon *Urera baccifera, Coilotapalus obtusa,* and young *Ceiba* sp. Lichens and algae on the bark of these trees supply food. It is quiescent during the heat of the day, and retracts into its shell when gathered. Emergence and active creeping follow brief confinement in a moist glass jar. In nature the temperature to which they are exposed is quite high, our data showing noontime temperatures (shaded-bulb thermometer) of as much as 32° C; the snails are found exposed to the sun. In the laboratory, at temperatures 28° to 30°C, they are uniformly *negatively* heliotropic at all times of the day or night. Accompanying this mode of response is a very definite reaction (retraction of head and cephalic tentacles, cessation of creeping) which results from abrupt shading. This correlation is shown by a number of other snails, and also by *Onchidium*. In such cases (9, 18, 3), since no response (other than the phototropic) is obtained from an increase of light intensity, it is, of course, impossible to suppose that the phototropic orientation is due to "shock reactions" determined by decrease of light intensity.

The reaction-time to shading in daylight (retraction of the tentacles) is 0.46 secs. ± 0.02, at 30°C.

In darkness, observed by intermittent use of non-directive light of low intensity, *Liguus* orients upward upon a sloping glass surface. Although there are brief intervals, with a few individuals, in which downward creeping was observed, the orientation is by vast preponderance upward. This corresponds to the positions observed in nature. We were quite unable to find indications that *Liguus* ever creeps downward for any great distance. Possibly they return to earth only when blown off the trees.

The upward orientation is definitely of the character described for *Agriolimax*. The vigorous creeping of *Liguus* makes it favorable for such observations. Employing the notation earlier described, we found, for a group of three individuals, with a surface of slope $\alpha = 25°$, $\theta = 65°$; $\alpha = 75°$, $\theta = 80°$; $\alpha = 90°$, $\theta =$ 88°-90°. Using the procedure discussed by Crozier and Pincus (33), we found that a low value of θ exhibited on a surface of slope $\alpha = 25°$ was immediately altered to $\theta = 65° \pm$ when the slope was increased, without handling the animal. When the glass plate is immersed in a tub of water, by carefully lowering the plate while keeping it horizontal, then subsequently tilting it, it was found that θ was invariably *less* under water than with the same slope in air. The angles were observed by the aid of coordinate lines marked on the glass plate.

In view of the phototropic behavior of *Liguus,* these experiments, as well as those considered later, were repeated with individuals from which the cephalic tentacles (with the eyes) had been removed. No qualitative differences could be detected.

The speed of progression increases, as Cole (7) found with *Helix,* when α is increased. With weights added, by attachment to the shell, the speed of movement decreases (cf. 24); this is seen "naturally" as well, when temporary chains are formed by the creeping of one individual upon another—creeping then usually stops (if the surface be vertical).

Qualitatively, it was easy to demonstrate that compounding of the orienting influences of gravity and of (lateral) illumination are as apparent here (and in *Onchidium*) as in slugs (78, 37, 21).

When *Liguus* is creeping on a vertical surface, the axis of the shell is exactly parallel to (although not coincident with) the chief axis

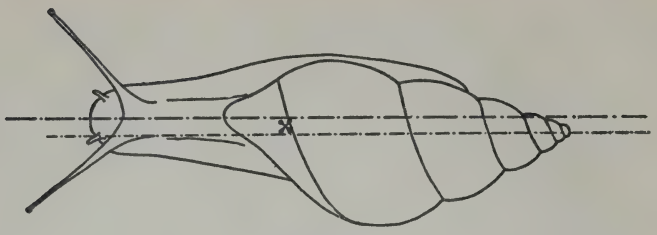

FIGURE 2

Outline of *Liguus* during geotropically-oriented vertical creeping. The chief axis of the body and the axis of the depending shell are parallel. The cross marks the center of rotation during orientation. (About natural size.)

of the body (Figure 2). A thread is easily attached by wax to the apex of the shell. This permits gentle manipulation of the shell without putting undue stresses on the muscular attachments of the shell, since a pull too sudden, or too severe, simply detaches the thread. If the shell is lifted slightly and pulled to one side, creeping continues without pause, but orientation promptly occurs in such a way as to bring the body into direct alignment with the axis of the shell as imposed by the pull. Similar results attend comparable pulls to one side during creeping upon a horizontal surface, as already seen by Cole (7) in *Helix*. These effects are not to be strictly compared with the homostrophic effect described in annelids and arthropods (55, 56, 57, 26); as suspected by Cole (7), mere bending of the foot and *body* of *Liguus* to one side, either at the head-end or at the level of attachment of the shell (if the foot has regained adherence), does not result in self-straightening of the body. This is most easily tested by permitting the snail to creep over a thin piece of stiff paper partly overlain by another; when passing over the edge of the upper paper, the body can be bent by suitable movement of one piece, the foot remaining attached to both. There does not result any modification of the direction of progression, but the anterior portion keeps steadily on its straight way.

If the vertical glass plate with an upward-creeping snail be rotated swiftly but smoothly through an angle, in the vertical plane, the snail may cease creeping for a moment; pedal waves usually cease, but otherwise no notable change is visible on the foot. This pause may be very brief. When the plate is turned so that the axis of the foot is horizontal, the shell, beginning almost immediately, begins to swing downward; it rotates through an angle of about 45°,

never more. The speed of this rotation is at first high, then diminishes. The head-end, after a measurable latent-period, swings in the opposite direction, and the anterior half of the body turns until aligned with the axis of the shell, which during this process comes again to point straight downward. The center of rotation of the shell is between the planes of the longitudinal axes of shell and foot—coinciding, in other words, with the center of the visceral stalk.

Tracings from photographic records taken at successive steps in an orientation experiment of this kind are given in Figure 3. The

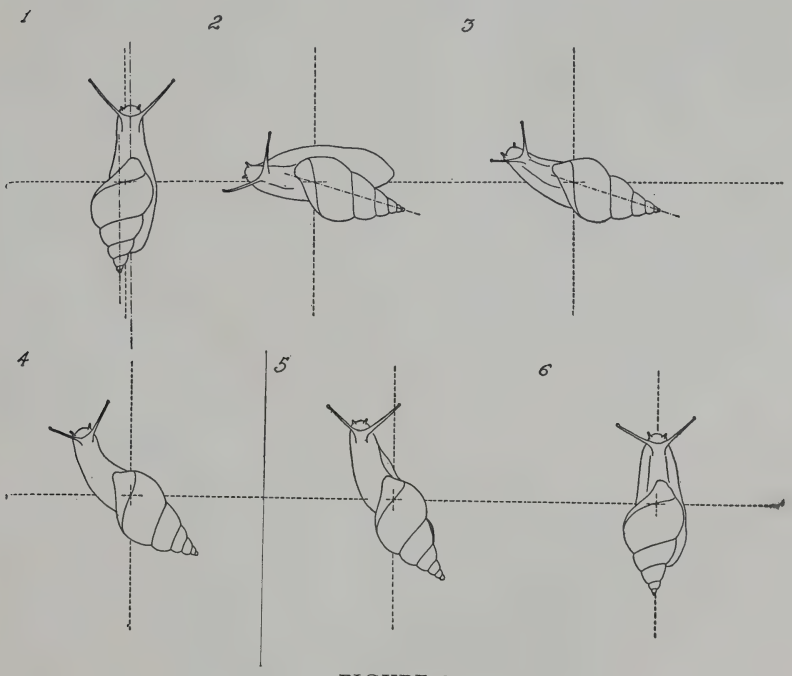

FIGURE 3

The process of geotropic response in *Liguus* as result of a turning of the axis through a right angle, in the vertical plane. The outlines are traced from photographs of successive stages, the center of rotation being brought to the same horizontal line in each case, so that comparison is made simpler. Actually there is, of course, forward movement such that the animal is moving upward at the same time as it is turning. One of the stereoscopic photographs is reproduced in Figure 4.

Between *1* and *2* the vertical plate is turned counterclockwise through 90°. The first evidence of geotropic response, sinking of the shell and beginning of upward turning of the head, has been caught at *2*. The interval between *1* and *2* is about 6 or 7 seconds. The succeeding steps, up through *6*, occupy about 2 minutes.

movement is slow enough to permit "motion picture" recording with a film-pack (cf. 8). A stereoscopic camera was employed, so that details of the movements could be studied at leisure. One such photograph is shown in Figure 4.

In the initiation of orientation the perceptible movement of the tentacles is not begun before that of the whole head. It might have been expected that the movement of the plate might stimulate through air currents, as is the case in turntable experiments unless precautions are taken to avoid it. But in these tests it was found that the same results were obtained after removal of the tentacles.

Various modifications of these tests showed that there is always and invariably a definite correlation between the direction and the balance of the pull of the shell and the process of orientation of the snail. One of these may be cited in detail.

It is a simple matter to force *Liguus* to creep downward, on a vertical surface, by properly adjusting the pull upon its shell. Detailed records from an experiment of this kind are given, from the photographic records, in Figure 5 (cf. also Figure 6), which depicts 13 successive steps in the same test. It is important to realize that throughout this experiment the pull upon the shell was at no time vigorous. If a load as much as 5 gms. be attached to the shell of a *Liguus* of ordinary size (mass 15 gms.), it will creep *downward* (or cease creeping). In these tests the pull was carefully adjusted so as to just barely "hold" the weight of the shell, and it

FIGURE 4
PHOTOGRAPH OF *Liguus* DURING GEOTROPIC ORIENTATION
See Figure 3

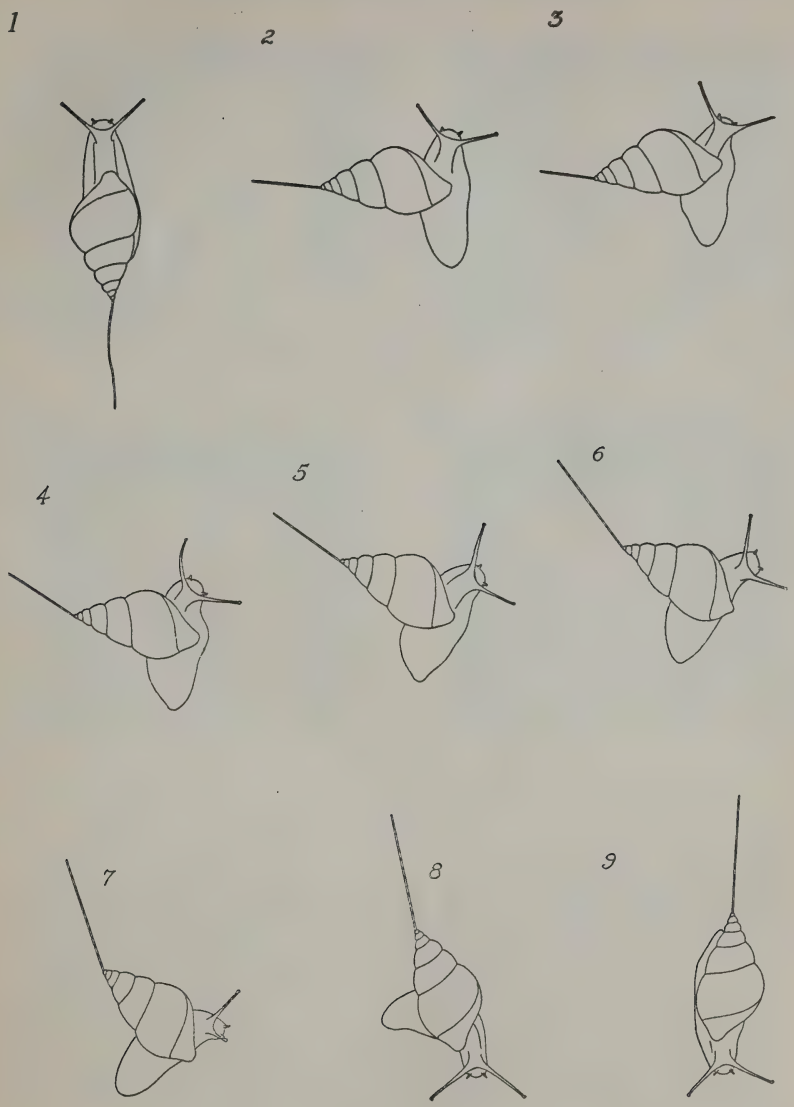

FIGURE 5

Tracings of photographic records of *Liguus* on a vertical surface, orienting in response to the adjustment of tensions in the columellar and parietal musculature. See Figure 6, and text.

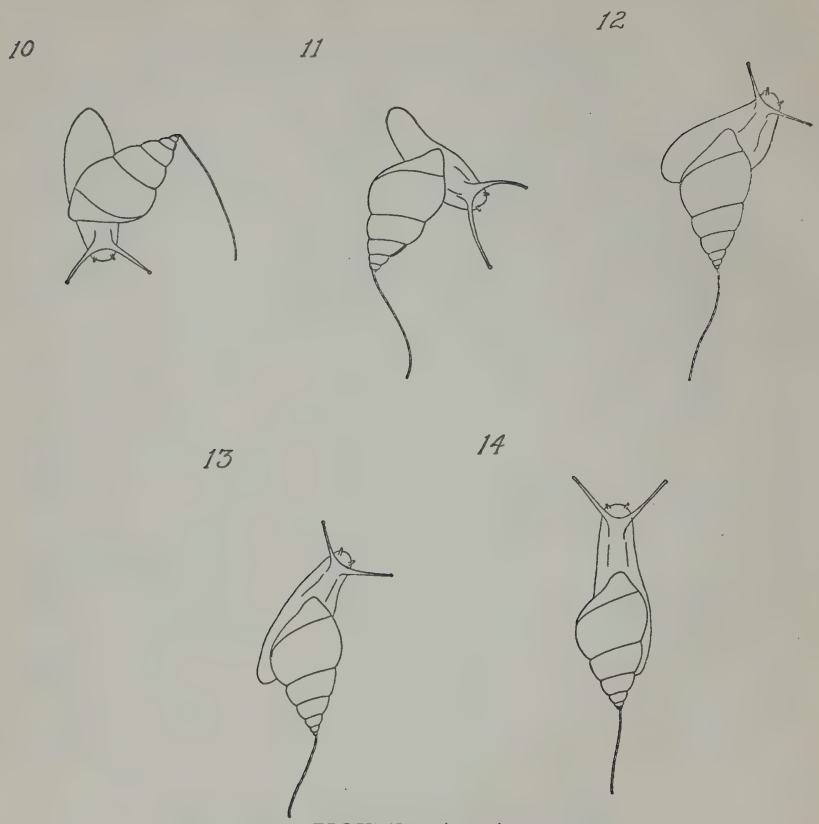

FIGURE 5 (*cont.*)

will be observed that the orientation is always *toward* the stretched side, not away from it as is the case in reversal of geotropism under excessive loads. In the experiment recorded in Figures 5 and 6, the snail is at first (*1*) creeping upward, the attached thread dangling freely. The shell is then pulled to the left, the tension being kept as even as possible during the subsequent orientation (*2* to *9*) which ultimately leads to straight downward progression; at *10* the string was released. The shell swings over to the right (*10*); the animal promptly orients upward.

Facts of this sort are difficult to account for on the basis of a control of the orientation by way of a statocyst. They were exactly reproduced in experiments with *Cerion* and with *Pleurodonte,* which are large enough to permit the requisite manipulation.

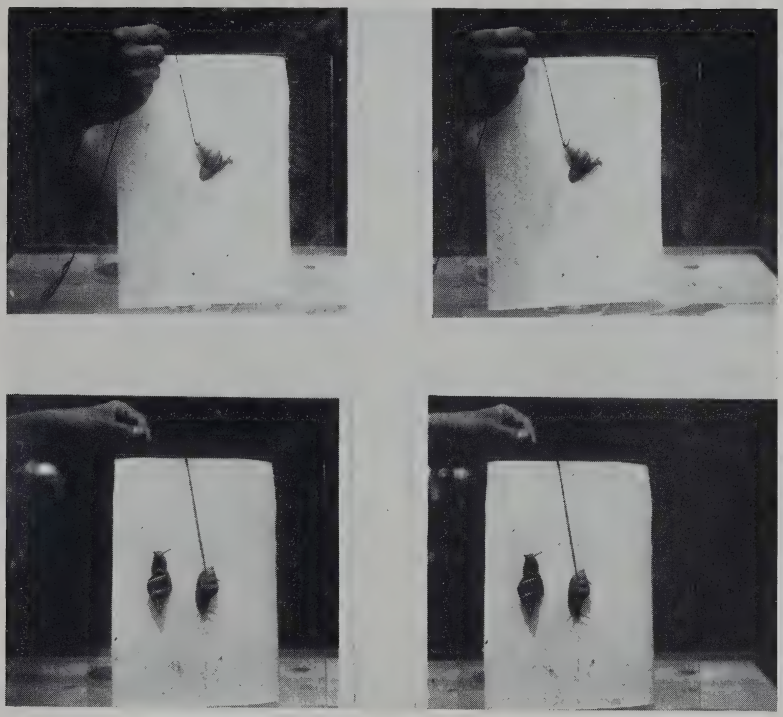

FIGURE 6

STEREOSCOPIC PHOTOGRAPH OF *Liguus* DURING ORIENTATION RESULTING FROM IMPRESSED MUSCLE TENSIONS

Cerion is an active creeper at night. It was caused to emerge by dipping briefly in water. On a vertical surface, the animal creeps straight upward. The shell, which is quite large, relative to the bulk of the animal, hangs straight down. The shell appears to be brought forward by "steps," due to the rhythmically intermittent contractions of the columellar musculature. The animal is very sensitive to slight mechanical disturbances; creeping stops, even though the tentacles may not retract. To quite moderate but abrupt reduction of light intensity the response is very clear—the cephalic tentacles are retracted, the head region shortens, the shell is "jumped" forward. This type of reaction must be avoided in tests of geotropic orientation.

On a glass plate the adherence of the foot is better than on moist wood. During active progression on glass the characteristic noise made by the foot of a number of pulmonates may be clearly heard.

If the plate is slowly and carefully turned to one side, the shell slowly drops, and, keeping pace with this, the animal orients so as to turn upward. When a thread attached to the shell is so held that the shell is gently pulled upward, *Cerion* creeps straight down. If

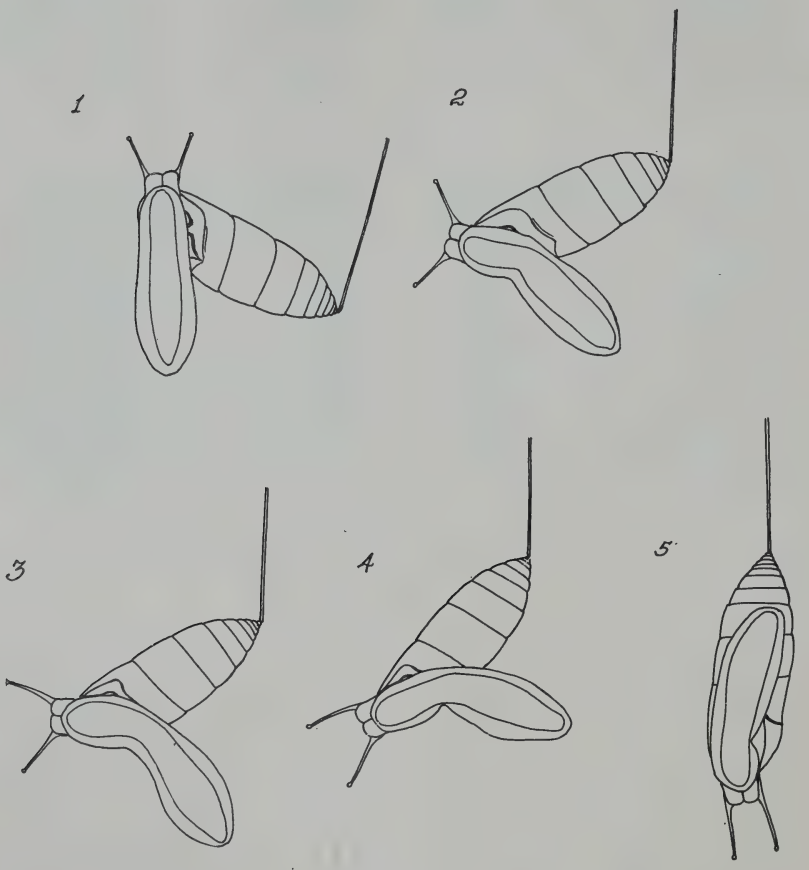

FIGURE 7

Orientation in *Cerion* as result of distorting tensions applied by a pull on a thread (cf. Figure 5). Successive stages from free-hand sketches. The animal is drawn as viewed through a glass plate to which the foot is attached.

the thread be then released, the shell topples over to one side, usually to the left, and the animal promptly turns upward and toward the opposite side. Certain details are given in Figure 7. When the shell is pulled to one side, as by an attached thread, and held so for a little while, muscular "set" occurs, so that the shell keeps its position for an appreciable interval after the pull is released.

Similar manipulation of the shell when on a horizontal surface leads to the same sort of forced orientation. This is not so much induced by the introduction of a twist in the columellar stalk, as to the change in pull (tension) when the posture of the shell is altered. The results as regards orientation are in no sense due to the slipping of the foot.

Rather rarely one sees spontaneous downward creeping, in *Cerion*. The shell is then hanging downward, its axis parallel to that of the foot. The shell then appears evenly balanced. When it topples to one side, as in these cases it ultimately always did, upward orientation promptly resulted (cf. Figure 8).

FIGURE 8

Temporary *geopositive* creeping in *Cerion*. At the moment of recording the snail has begun to turn upward, since the originally symmetrical posture of the shell became disturbed by a slight turn to one side; orientation promptly followed in the opposite direction.

Closely related to the foregoing is the fact disclosed in experiments by H. Federighi (in course of publication) upon the behavior of the marine snail *Urosalpinx* in water-currents. The rheotropic orientation of this snail results in creeping up-stream in a steady current, at a speed which is the same regardless of the velocity of flow of the water; but when the act of orientation itself is measured, it is found that the posture of the snail during turning, defined by the amount of turning per centimeter of path travelled while orienting, is di-

rectly proportional to the pressure exerted by the current on the cross-section of the snail's body (that is, to the square of the velocity of flow). It will be observed that this constitutes an effective parallel for the state of affairs discussed in the present paper.

When the plate bearing a *Liguus* is put under well-aerated water, in a deep tub filled with rain water at room temperature, the snail continues for some time to creep well. Surfaces of wood were also used. If sufficient air is in the lung, the animal may float in water. But if it is attached, the shell hangs down. The tentacles are not completely everted. After 20 minutes under water, or a little longer, so much slime has formed on the surface of the snail that its foot adheres with difficulty. Before this the tests were discontinued. Under water on a sloping or vertical plane, *Liguus* orients upward. If a cork float be made by trial of such size as just to carry the shell vertically, and is attached by thread and wax (or tape) to the apex of the shell, the distribution of tensions in the body due to the pull of the shell is then exactly as in the case of the previous tests in air. The snail should then orient and move *downward,* and in every case indeed does so.[2] It goes straight down to the bottom and stays there. It is rather difficult to conceive a mechanism dependent primarily upon a statocyst which would lead to this result. For it will be noticed that even if the reaction described is supposed to involve the kind of reversal of geotropism due to excessive pull (out of the question in any case), which is subsequently discussed, one does not thereby evade the difficulty. It may be pointed out, moreover, that such effects remove the possibility of interpreting the upward movement under water, in the absence of the float, as due to a "seeking of the air."

V

The analysis of geotropic orientation in young rats (33, 73) has led to the possibility that the amount of the upward orientation, or rather the position of the animal's axis in space, is determined by the

[2]A different result has been reported by Fraenkel (43) with a species of *Littorina;* he attached a small float to the shell, and found the snails still to move upward. With forms on the New England coast we have not been able to confirm this. *Littorina* is often a slow creeper, and even in the absence of light its geotropism easily undergoes reversal. We shall consider this case subsequently in detail. It is possible that too severe an upward pull leads to a reversal of orientation analogous to that discussed in the present paper for *Liguus.*

adjustment of tensions in the supporting elements of the two sides of the body in such a way that (within a threshold difference) an equivalent number of tension-receptors is stimulated per (gross) unit of time on each side. The skeletal muscles of vertebrates contain end-organs whose homologous stimulus is provided by stretch of the muscles (74, 4, 52, 42, 1). This might make possible, with suitable material, the investigation of the problem of "number of end-organs" concerned, by means of measurements of reaction-times or latent periods with differing magnitudes of the gravitational pull. But with the young rats this is impracticable because of the interference of the phenomenon of periodic activity. With such forms as the gastropods, however, this difficulty does not present itself, and the results given in the foregoing section plainly suggest the propriety of the desired tests in their case.

This was carried out by having *Liguus* creep upon a moistened glass plate held vertically, on which upward progression was obtained, and then rotating the plate in its own plane through an angle of 90°, so that the snail's axis was suddenly, but smoothly, turned to be horizontal. The time was then taken with calibrated stop-watches until the first visible sinking of the shell (about 1 sec. or *less*) and, more significantly, until the initiation of the turning of the head. The starting point of the time-measurement was taken when the axis of the snail was at about 45° to the vertical, in other words at the mid-point of the 90° rotation. Errors arising in this connection are of completely negligible magnitude.

From any single individual there was obtained in this way a remarkably consistent series of readings. In general, the differences between mean reaction-times when *orienting turns* were (*a*) made to the right and (*b*) made to the left were without significance. Occasionally those to the right gave slightly higher reaction-times than those to the left. We looked for some such effect on account of the fundamental torsion of the snail's body, but the most extreme cases found were these:

Reaction-time, orienting to the right.....4.91±0.30 sec.
Reaction-time, orienting to the left.....4.40±0.35 sec.
Reaction-time, orienting to the right.....4.44±0.22 sec.
Reaction-time, orienting to the left.....3.53±0.12 sec.

The reaction-times, or more properly the *latent periods* for perceptible initiation of geotropic response, ranged in different cases from 3.60±0.12 to 7.63±0.58 sec. The animals used weighed 10.0 to

15.3 gms. (including the weight of the shell). The mean reaction-time for all was 4.92 secs.

Several aspects of this definite latent-period present themselves for analysis. At the moment we are concerned merely with the essentially qualitative proof that one may by its use obtain evidence concerning the muscle-stretch *vs.* "statocyst" conception of geotropic excitation. The latent period is clearly a complex quantity. If the gravitational pull supported by the dermo-muscular sac of the snail's body affects its contained stretch-receptors, it might be considered to produce, in the central nervous system, an effect leading to orientation through asymmetric tensions reflexly established; and the speed of establishment of the central effect might depend upon the intensity of excitation. Again, if the case be at all comparable with that in mammals, the proprioceptive tension-excitations may reflexly determine tonus differences on the two sides, or rather the development of tension on one side with reciprocal relaxation on the other (22, 23), while the speed with which this development becomes apparent is chiefly governed by the state of extension of the tension-developing elements. Without other information, it might thus still be held that a statocyst determines the orientation, and even its speed of appearance, the reaction-time being determined merely by the delay, variable according to the degree of stretch, which must intervene before the orienting muscles can be thrown into play. But this additional information we already have, because it has been shown previously that the *direction* of orientation is determined by the direction of pull. Hence we may quite properly seek for a connection between gravitational excitation and latent period, with the rôle of stretch-receptors directly in view.

Of the several modifications of the experiment described, which will subsequently be discussed quantitatively, we mention these: (1) the angular displacement during the rotation may be varied, also (2) the slope of the surface on which the animal rests; (3) the weight supported by the columellar muscles may be increased, by attaching masses to the shell; (4) this pull may be decreased, by putting the animal under water; (5) it may be decreased radically by surgical removal of the shell. If the control of geotropic orientation and of the latent period of the geotropic response is determined by the excitation of stretch-receptors, we have definite expectations in each of the circumstances enumerated. (1) We look for the reaction-time to be greater with lesser angular displacement, and also (2) with

decreased slope of the supporting surface; in (3) we expect the reaction-time to be decreased, and as a function of the attached weight; so also in (5), though the removal of the shell and a portion of the visceral mass is not without objectionable features; in (4) we expect (*a*) to find the reaction-time increased with the snail under water, and (*b*) that it can be caused to diminish by the attachment of additional loads.

It may be said at once that each of these six expectations have been verified in the course of the experiments. The length of the latent period is such that a certain leeway is afforded in the attachment of weights. We will deal with several individual snails, in detail; it is not permissible to average the results from all because not every snail was used in every experiment, and the degree of reactiveness is not the same in all. Yet we would state, as clearly as possible, that, with the 15 *Liguus* used in the detail experiments, every sequence of observations gave results consistent with those obvious in the figures here given.[3] The experiments discussed refer to displacements on a vertical surface, rotated in its own plane; 10 to 22 readings—with adequate rest periods in between—were taken for averaging; none have been rejected; the probable errors of the means have been corrected for sampling.

Liguus

Latent period, in air; rotation of 90°.....3.76±0.21 sec.
Latent period, in air; rotation of 45°.....4.67±0.66 sec.

The mean latent period for 15 individuals, in air, reacting as a result of a 90° turn, was 4.92±0.08 sec.; for 5 individuals under water, 9.54±0.20 sec. The water was at 29°C., and tests were confined to the first 15 minutes or less of immersion. For additional

[3]Lest someone be tempted to suggest that in these cases the snail "learns" to react faster when loads, for example, have been added, we may state that control tests were made after the removal of the weight. Sometimes the reaction-time was slightly *longer* than before experience with the added load. This is probably connected with an effect upon the speed of progression, but may represent fatigue. There are "facilitation effects," however, which must be guarded against. If a *Liguus* is several times and in fairly rapid succession rotated on its vertical plate to the same side, it shows for some minutes a persistent tendency to travel to that side, which, however, soon wears away. Anyone who wishes to do so may, of course, term this "learning," although it is clearly a mere "persistence" of a reaction once started, and which is greater with added loads; unless care is taken to avoid its influence it can give curious results in the apparent latent period. It is easily circumvented by permitting full orientation in response to one turn before another is impressed.

pull, small bits of lead were attached by wax to the frontal side of the shell near its apex.

Liguus, No. 13.	
Latent period, in air	4.34 ± 0.16 sec.
Latent period, under water	7.57 ± 0.21 sec.
Liguus, No. 14.	
Latent period, in air	3.84± 0.28 sec.
Latent period, under water	7.70 ± 0.66 sec.
Liguus, No. 11.	
Latent period, in air	6.39 ± 0.41 sec.
Latent period, in air + 0.35 gm.	4.77 ± 0.11 sec.
Latent period, in air + 2.45 gms.	4.52 ± 0.20 sec.
Latent period, in air + 5.56 gms.	4.12 ± 0.10 sec.
Liguus, No. 2.	
Latent period, in air	5.33 ± 0.33 sec.
Latent period, in air + 1.5 gms.	3.98 ± 0.13 sec.
Latent period, in air + 3.0 gms.	4.01 ± 0.13 sec.
Liguus, No. 12.	
Latent period, in air	6.46 ± 0.20 sec.
Latent period, under water	12.2 ± 0.66 sec.
Latent period, under water + 2.3 gms.	5.43 ± 0.31 sec.
Latent period, under water + 4.7 gms.	2.60 ± 0.26 sec.
Liguus, No. 1.	
Latent period, in air	7.63 ± 0.58 sec.
Latent period, under water	11.23 ± 0.38 sec.
Latent period, under water + 2.3 gms.	6.18 ± 0.30 sec.
Latent period, under water + 4.7 gms.	3.20 ± 0.10 sec.

There is a perceptible tendency for the *variability* of response to decrease with increase of tension. Subsequent work with other forms has shown this to be a real relationship. Exact comparisons among the numbers obtained in these tests cannot be made, because of variable weights of individuals, the variable amounts of air held in the lung and other uncontrolled conditions. But it is perfectly clear that the latent period for geotropic response is specifically a function of the pull which the columellar musculature supports and transmits to the dermo-muscular body-wall. This is true whether the pull is increased or decreased—the reaction-time varies reciprocally. When the shell with a portion of the visceral mass is removed, by first tying a waxed ligature about the columellar stalk, then cutting above it, the internal fluid pressure being thus retained, the animal recovers in an hour or two so that it creeps about rather sluggishly, but definitely. The foot adheres sufficiently to permit turning experiments. The sensitivity to mechanical disturbance appears increased, for a time, so that these tests were made inside a large closed glass jar. The latent period, in several different cases, averaged 14.0 to 16.5 secs.

The preliminary downward sagging of the body, preceding orientation, was quite apparent. These long reaction-times contrast sharply with those involved in reactions to shading (as already noted), namely 0.46 ± 0.02 sec., or to touch (in the case of the head region), and the contrast is entirely consistent with the notion that for the gravitationally induced orientation one does not have to do with a centrally significant sense organ, such as a statocyst might be, but rather with slower phenomena of distortion in muscle (and, or) skin. One has no difficulty, then, in arriving at a homogeneous view of both the primary excitation concerned and the limitation of the extent of upward turning.

There is a further complication to be recorded, however. When a *Liguus* is forced to bear an additional load during creeping, there is an upper limit to the load which can be attached while still securing geo-negative creeping. This upper limit is related to the size of the individual, being higher for the larger snails. The average "critical load" (in air) was about 5.8 gms., or a little more than a third of the mean weight of the animals. For smaller individuals the critical load was as low as 3.0 gms. With loads above these limits the foot does not become extended to the normal degree, often the cephalic tentacles are not fully everted, and when progression is evidenced the snail orients downward. In doing so it turns *away* from the stretched side. When rotated through 90°, in the vertical plane, it reacts as before described, but turns down instead of up. If the load be removed, it promptly orients upward. Similar facts have been obtained with *Helix*. It has been pointed out that the analysis in terms of tension-receptors need not exclude the participation of statocyst function in certain aspects of these geotropic orientations (cf. 16), and that particularly we may look for its influence in connection with the control of upward or of downward direction—and thus with the reversals of geotropism which occur "spontaneously" in these and in other forms, and which can also be induced experimentally. This need not be any more mysterious than, for example, strychnine-induced "reversals of inhibition" in galvanotropism (15, 17, 44), and they can indeed be approached on this basis. Nor need this indeed be any more mysterious than the very definite relationship between active *progression* in these forms and their geotropic *orientation;* the latter does not occur unless creeping is being carried out. In any case, the problem of reversal of orientation does not concern us at the moment.

What does concern us is the fact that definite relationships exist between the direction of the pull sustained by the columellar and body musculature, and the direction and the speed of orientation. These relationships are completely consistent with the interpretation that the primary geotropic excitation is due to the influence of the pull of the body-mass upon tension proprioceptors. In our opinion they cannot be interpreted in terms of stimulation of a statocyst. They therefore appear to flatly contradict the position taken by Piéron (70, 71).

VI

The connection between speed of progression and geotropic excitation, after orientation is attained, may permit obtaining data for the disentangling of the two elements involved in geotropic orientation, as we have already indicated. Moreover, these relations have an independent importance in relation to the problem of the statocyst.

For the foot of *Limax* certain rules have been deduced (28, 24), which subsequent study by one of us with Mrs. B. Daly, employing *Helix,* has fully confirmed. The speed of creeping (on a vertical surface, during upward, oriented geotropic progression) is directly proportional to the velocity with which a single average wave courses over the foot, and is independent of the weight of the animal, and of the dimensions of the foot (at constant temperature, within any one species); the velocity of progression is directly proportional also to the frequency of pedal waves (i.e., to the number making appearance on the foot per unit of time); the proportionality factor is independent of temperature, though increasing with the size of the individual, but the intercept constant is a definite function of temperature; the frequency of waves required to produce a given velocity of movement is an exponential function of the reciprocal of the absolute temperature; the velocity of creeping is likewise directly proportional to the advance of the foot due to a single wave, the proportionality factor here decreases with rise of temperature; but is independent of the mass carried, the intercept constant being proportional to the mass lifted. The lifting of attached loads during vertical ascension does not alter the law of linear proportionality between speed of progression and frequency and effectiveness of pedal waves. For equal velocities of ascension these quantities are the same, regardless of the weight being lifted, although small added loads

increase the speed of spontaneous upward creeping, larger ones decrease it.

We desired to see if such relationships might not hold with other forms as well. It will be noticed that in their terms the problem of the *speed* of oriented progression, as affected by geotropic excitation, cannot be investigated by mere superficial examination of the connection between speed of movement and stimulation, because the speed is related not only to the velocity and to the frequency of the pedal waves but also to their individual efficiencies. The latter might be *directly* affected by the response of the adhering mechanism of the foot when excess pull is experienced, and thus in a way not directly connected with geotropic excitation. None the less, if the intermittent contractions of the body- and columellar-muscles which advance the shell-mass by "steps" are involved in the conditions of symmetrical excitation by impressed tensions, and the frequency of these steps is connected directly with the speed of movement, the amount and the speed of geotropic orientation might well have very complex relations to the intrinsic mechanism of the foot.

The foot of *Liguus* is typically helicine; each wave, which runs from posterior to anterior end of the foot, covers the whole breadth of the foot, consists of a light-appearing zone preceded by a darker zone. Four or 5 such waves, evenly spaced, are apparent on the foot at one time. The foot measures in smaller specimens 27.5 by 8 mm. to 33 by 6.5 mm., in medium sizes ones about 42 by 13 mm. With speeds of vertical creeping between 0.08 and 0.14 cm. per sec., the frequency with which waves make their appearance varies only between 0.71 and 0.91 per sec., at 30°C. Measurements were obtained by counting the number of waves disappearing at the anterior end of the foot while covering a measured distance, the time being taken with a stop-watch; or by measuring the time for, and the distance covered by, a definite number of waves (10, 20, or 40). From these data it is possible to compute the *advance due to one wave* (A), the *frequency of the waves* (F), and the *speed of progression.* All measurements were taken at temperatures between 28.2° and 31.6°.

Figure 9 shows, for typical individuals, the relationships between speed of progression (V) and frequency of waves (F). It is to be remembered that slight errors of observation introduce noticeable deviations in the computed ratios. For the most part the speed of spontaneous creeping during vertical ascension goes through a maxi-

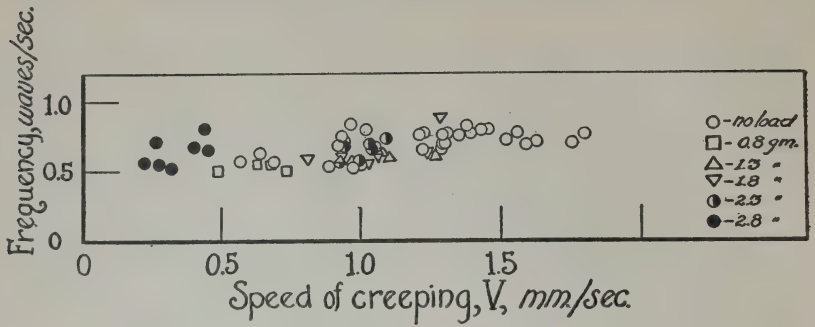

FIGURE 9

The speed of progression, in *Liguus*, as related to the frequency of pedal waves appearing on the foot. In contrast to the condition in *Limax*, while there is a slight tendency for increasing frequency to be associated with increase of speed, the effect is not pronounced.

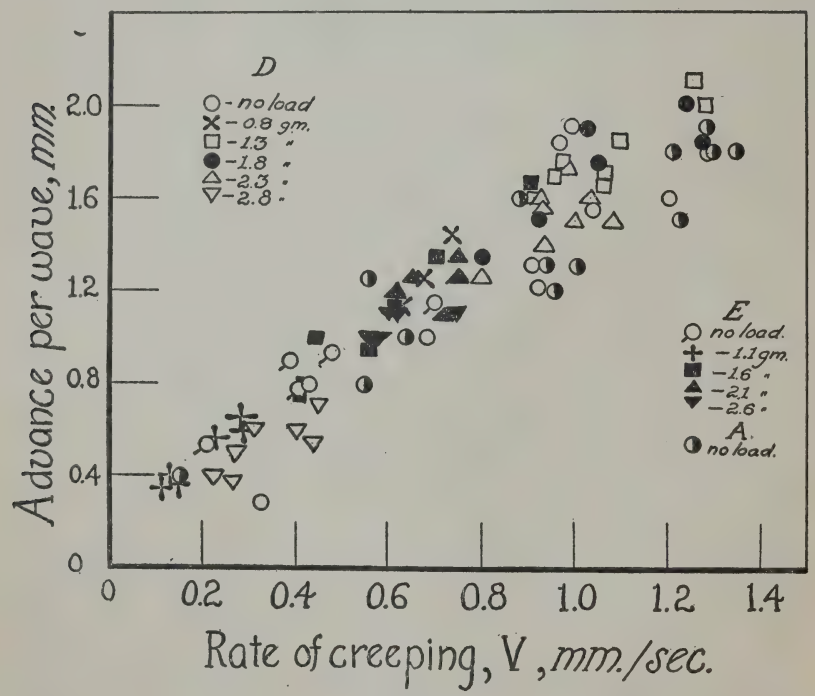

FIGURE 10

The speed of creeping V (in these cases, vertical upward progression) is in *Liguus* chiefly determined by the effectiveness of the individual pedal waves A rather than by their frequency (cf. Figure 9). Data from 3 animals (A, D, E), showing the constancy of this law of action of the foot and its independence of the load carried. In data from other individuals also it is found that the line connecting A with V is practically independent of the size of the snail.

mum, beginning at first slowly, increasing, then slowing again; this may be followed by another similar cycle. It is these spontaneous changes of pace which have been timed. By stimulatory manipulation it is possible to cause brief accelerations of movement, but the fact that both the magnitudes of the single pedal waves *and* their frequencies are variable, and in a measure independently, would make this procedure undesirable for the present purpose.

It is seen that over a relatively large range of velocities of movement the frequency of pedal waves does not alter materially. This is in rather striking contrast to the situation in *Limax*. With the latter (28, 22) this frequency changes more decidedly than does the advance due to a single wave. From Figure 10 it is quite clear that the reverse is true of *Liguus,* and that in the latter the locomotor efficiency of the waves is the chief variable determining the speed of movement. This contrast, which has been developed by the investigation of the foot in various other snails, appears unquestionably to be related to a characteristic difference in the structure of the foot in the two cases. In *Limax* the locomotor waves appear only on the central longitudinal area of the foot, its lateral fields being the organs of adherence. The helicine foot, however, forms the pedal waves over its whole breadth, so that adherence and progression are not attended to by functionally separate divisions of the pedal surface.

For reasons entirely analogous to those making it impossible to measure the temperature characteristic for velocity of a single pedal wave in *Limax* (28), it is impossible to do very much analytically with the relations between load carried (i.e., tension) and speed of progression. As in *Limax,* for equal velocities of progression the frequency and the efficiency of the pedal waves are the same regardless of the load carried in vertical ascension. In general, small loads tend to increase the speed, larger ones to depress it, but the relationship is not exact.

VII

Summary

The upward, geotropically-oriented creeping of typical tree-snails such as *Liguus, Pleurodonte,* and others, including *Cerion,* is governed by the sensible equality of tensions produced through pull of the body upon its supporting elements. This is attested by the control of orientation at will by suitable distortions of these tensions. Interconnections with the mechanism of progression in creeping are

discussed. It is pointed out that in *Liguus,* for example, the advance due to one pedal wave is directly proportional to the speed of creeping, the frequency of the waves (in contrast to the situation in *Limax*) being only slightly modified at different speeds of progression. It is not possible to deal, in general, with relations between tension and speed in any simple way. Under water, *Liguus* creeps upward on an inclined surface; but orients *downward* if an appropriate float gently pulls the shell upward. The latent period for geotropic response, under standardized conditions, in *Liguus,* is a function of the pull which must be supported. An added load reduces it; it is lengthened under water; added loads again reduce it, under water. With the exception of a possible but unproved relationship to *reversals* of orientation, such as may be induced by supra-optimal loads, there is consequently no room for the assumption that geotropic orientation in these forms is governed, either as to *direction,* to *amount,* or to *latent period,* by the involvement of the statocyst. These facts are entirely consistent, however, with the notion of proprioceptive stimulation, through impressed tensions as the controlling feature in the gravitationally excited orientation of gastropods (Cole, Wolf, and others); the forms mentioned, as also *Onchidium,* likewise show definite relationships between the extent of upward orientation during steady progression and the slope of the supporting surface.

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L'ORIENTATION GÉOTROPIQUE DE GASTÉROPODES

(Résumé)

La réaction négativement géotropique de mollusques typiquement arboricoles (par exemple: *Liguus, Pleurodonte,* etc., y compris *Cerion*) est déterminée par l'équalisation des tensions induites par la pesanteur du corps et les effets de traction en résultant pour les éléments de support. Ce point de vue est démontré par la possibilité de contrôler l'orientation par des modifications appropriées apportées à ces tensions. Les connexions pos-

sibles de ce phénomène avec le mécanisme de progression pendant la reptation sont discutées. Il est mis en évidence par exemple, que chez *Liguus* l'avance due à une onde pédieuse est directement proportionnelle à la vitesse de reptation, la fréquence de ces ondes étant modifiée d'une façon à peine sensible, pour différentes vitesses de progression (en opposition avec ce qui se constate chez *Limax*). Il n'est pas possible de déduire des relations simples entre tensions et vitesses de progression.

Sous l'eau, *Liguus* est négativement géotropique dans sa progression sur un plan de verre oblique; sa reptation est au contraire *descendante* si un flotteur approprié tend à soulever délicatement la coquille vers le haut. Le temps de latence pour la réponse géotropique chez *Liguus,* dans des conditions bien standardisées, est une fonction de la traction effective. L'addition de poids réduit ce temps; le fait de placer l'animal sous eau l'allonge; l'addition de poids à la coquille de l'animal immergé le réduit à nouveau. Si l'on exclu la possibilité—du reste très peu probable—d'une relation impliquant le *renversement* de l'orientation, par exemple par addition de poids anormalement élevés, il nous semble qu'il ne reste aucune place pour attribuer l'orientation géotropique déterminée, en ce qui concerne la direction ou le *degré d'orientation* ou le *temps de latence,* à l'existence ou au jeu de statocystes.

Ces faits sont au contraire parfaitement consistants avec la notion de stimulation proprioceptive, par l'intermédiaire des tensions développées dans l'organisme, comme agent déterminant l'orientation géotropique de gastéropodes (Cole, Wolf, et al.). Les formes considérées, aussi bien qu'*Onchidium,* montrent des relations bien définies entre le degré d'orientation pendant la progression et l'inclinaison du plan-support. Ceci est donc en contradiction avec le point de vue défendu par Piéron, basé sur des expériences qu'il nous est facile de démontrer inadéquates en bien des points.

CROZIER ET NAVEZ

DIE GEOTROPISCHE ORIENTATION DER GASTROPODEN

(Referat)

Das geotropisch orientirte Aufwärtskriechen der typischen Baumsehnecke wie *Liguus, Pleurodonte,* und andere, auch *Cerion,* wird durch die sensible Gleichheit der Spannungen beherrscht, und durch die anziehande Wirkung des Körpers auf das ihn tragende Element verursacht. Dies wird durch die willkürliche Kontrolle der Orientation durch geeignete Verdrehungen der Spannungen bewiesen. Gegenseitige Beziehungen mit dem Mechanismus der Fortbewegung beim Kriechen wird erörtert. Man weist darauf hin dass in *Liguus* z. B. die Fortbewegung, die durch eine wellenförmige Bewegung des Fusses bewirkt wird, in direktem Verhältnis zu der Schnelligkeit des Kriechens steht, und dass die Häufigkeit der Wellen (im Gegensatz zu *Limax*) durch die variable Schnelligkeit der Fortbewegung nur wenig verändert wird. Es ist, im allgemeinen, unmöglich die Beziehungen zwischen Spannung und Schnelligkeit in einfacher Weise zu behandeln. Unter Wasser kriecht *Liguus* aufwärts auf einer schiefen Ebene; doch wendet sich *abwärts* wenn die Muschel durch einen schwimmenden Korken sanft emporgehoben wird. Die Latenzperiode für geotropische Reaktion unter normalen Umständem in *Liguus,* ist eine Funktion des Ziehens, das jedoch unterstützt werden muss. Eine vergrösserte Last reduziert es; unter Wasser es verlängert wird; eine vergrösserte Last reduziert es auch unter Wasser. Mit Ausnahme der möglichen, doch unerwiesenen Beziehungen zu *Umkehrungen* der Orientation, wie sie durch supra-optimal Lasten herbeigeführt werden können, ist folglich kein Grund für die Annahme

vorhanden dass geotropische Orientation in diesen Formen, entweder in Betreff der *Richtung des Betrages,* oder der *Latenz Periode* durch die Verwicklung mit einem Statocyst beherrscht werde. Diese Tatsachen stimmen völlig mit der Idee der propriozeptiven Stimulation durch aufgedrückte Spannungen als der beherrschende Faktor, der durch die Gravitation erregte Orientation der Gastropoden (Cole, Wolf, und andere); die genannten Arten, zowie auch *Onchidium* zeigen auch bestimmte Beziehungen zwischen der Ausdehnung der Orientation nach oben während stetiger Fortbewegung und der Neigung der stützenden Fläche. So fand man genügenden Widerspruch gegen die entgegengesetzte Ansicht, die Piéron vertritt auf Grund von Experimenten die, wie man andeutet, in mancher Hinsicht unzulänglich schienen.

Crozier und Navez

INDIVIDUAL DIFFERENCES IN MOTOR SKILLS*[1]

From the Psychological Laboratories of Stanford University

ROBERT HOLMES SEASHORE

Degree of skill or efficiency in a given form of overt sensori-motor coordination may be objectively measured by the number of units of work done correctly per unit of time. This measure is in terms of a single integrated score rather than of separate scores for output, accuracy, or time. An alternative measurement of skill is of the type suggested by Pear (25), who emphasizes the ease of efficiency of performance where rate is constant.

This research attempts to evaluate the methods of theoretical and job analyses in relation to methods of selecting personnel for motor skills. The improvement of training methods, mechanisms, and working conditions is touched upon only indirectly. Three questions may well be asked concerning the methods of personnel selection: (1) What type of units or variables may be used in theoretical and job analyses of skills? (2) What validity may be expected of selection methods as a result of findings on the persistence of individual differences through training? (3) What techniques of measurement should be used to accord with findings as to the specific or general nature of motor skills? In consideration of the relatively small amount of work which has been done on motor skills, a fourth question may be still more important, namely: What lines of research on motor skills are likely to be of most importance in the near future?

HISTORICAL SUMMARY

In the description and analysis of individual differences in com-

*Received for publication by Walter Miles of the Editorial Board, June 21, 1928.

[1]This research is one of a series on motor skills carried out by the writer, while a National Research Council Fellow, under the direction of Professor Walter Miles at Stanford University. This line of investigations has grown out of the studies on serial motor performances in the Iowa Laboratory under Dean Seashore and from the motor apparatus and cycle method of testing developed by Professor Miles. The writer wishes to express his appreciation to these men and to Professor L. M. Terman and Professor Paul R. Farnsworth of Stanford University for aid in the research.

plex motor performances four general hypotheses have been proposed. In these hypotheses individual differences in motor performances are variously considered to be determined by: (1) a general motor function and other special abilities, (2) a number of simple variables such as speed of reaction-times, reflexes, steadiness of hand, etc., (3) a number of serial motor coordinations, called "basic motor capacities," or (4) differences in training, both direct and transferred, and including informal types of training.

General Motor Ability. As to the existence of a "general motor ability," four researches may be cited, those of Perrin (26), Muscio (23), Garfiel (11), and R. H. Seashore (32). Both Perrin and Muscio conclude that we may not speak of "motor ability" on account of the low intercorrelations found between various motor performances. Perrin pointed out the influence of "technique" or method of work upon scores in complex performances but concludes

TABLE 1

INTERCORRELATIONS OF MOTOR TESTS (PERRIN, 26)

(N=51, undergraduates, both sexes)

	Bogardus*	Card-sorting†	Coordination†
Complex motor tests			
1) Bogardus			—.10
2) Card-sorting	—.06		.36
Simple motor tests			
1) Reaction-time	.16	.21	—.06
2) Aiming	.24	—.09	.12
3) Balancing	.38	—.06	—.11
4) Tapping	.22	.02	.01
5) Steadiness	.09	—.04	.01
6) Tracing	—.11	—.01	.09
7) Dynamometer	—.22	.20	.39
Lung capacity	—.14	.09	.22
Army Alpha	.03	.02	.10
University grades	—.17	.01	.21
Simple color reactions and discrimination-and-choice color reactions			.59
Simple color reactions and discrimination-and-choice card reactions			.40
Simple color reactions and simple card reactions			.33
Speed in tapping and speed in simple color reactions			.06

*The Bogardus test (2) requires placing a small block on a pedestal with the right hand, pushing of block to edge of table by a rotating mechanical arm, where it is caught by the left hand, placed in chute leading to hopper from which the right hand picks up block to repeat cycle.

†The coordination test consists of moving a stylus in a triangular path with one hand, and another stylus in a square path with other hand. Rate of movement is one side of figure per metronome beat at high speed.

that the study of motor abilities awaits the formulation of the psychology of motivation. The intercorrelations of the motor tests used by Perrin are reproduced in Table 1.

Muscio (23) concludes as follows from data similar to Perrin's:

1) There is no "motor type." Terms such as "motor dexterity" or "practical ability" are misleading since a person's relative performance in any one motor test does not normally represent a general motor test performance level, that is, a relative performance level which will be attained in any other motor test.

2) Motor capacities are relatively independent of intelligence. For, since motor tests do not correlate with one another positively to any appreciable degree, they cannot, in general, correlate positively with any other tests, and therefore not with intelligence tests.

3) From the point of view of vocational guidance, the practical conclusion is that every occupation which consists mainly of a routine performance of specific movements will require specific vocational tests, that is, motor (vocational) tests for a given "motor" occupation must be tests of just those motor capacities that function in that occupation.

Garfiel (11) gave a large number of ordinary laboratory and simple gymnasium tests to 50 Barnard College girls and obtained intercorrelations and reliabilities of the separate tests, and also their relation to a criterion of "general motor ability" as rated by a group of gymnasium teachers and prominent students interested in that field. Intercorrelations of the separate tests ranged from small negative values to positive values as high as .44±.08. Correlations of the separate tests with the criterion showed practically the same range, with one test, the 100-yard dash, correlating .63±.06 with the criterion.

Selecting 8 of the 20 tests on the basis of intercorrelations with the criterion, a battery was formed which showed the following reliabilities and multiple correlations with the criterion:

	r	*P.E.*
First giving of scale with		
1) Second giving, i.e., reliability	.77	.04
2) First criterion (a measure of validity)	.77	.04
3) Second criterion (another measure of validity)	.73	.05
Second giving of scale with		
1) First criterion (another measure of validity)	.64	.06
2) Second criterion (another measure of validity)	.58	.07
First vs. second criterion ratings (reliability)	.92	.02

Garfiel's own conclusions are as follows: "that inasmuch as

"1. The central tendency of the intercorrelations of motor tests is positive (.15 to .25) and

"2. A group of people (the judges) found the term motor ability intelligible and meaningful, proving this by a self-consistency of .92 and

"3. That a team of tests could be found which would agree so closely (.77) with the combined ratings of the judges—in the light of 1, 2, and 3 we may conclude that the use of the term motor ability is justified and we may begin to investigate the relationship of this group of abilities to intelligence and to various special abilities for vocational guidance. And that we may begin at once to use the tests as a means of classification of students for work in physical education because they so nearly approximate in seventeen minutes the judgment of eight teachers and students after six months or more of acquaintance.

"For the present I should venture the guess that mental and motor ability are different groups of abilities which tend to a very low positive relation, say around .10 to .12 for (adult) people in general.

"This constitutes the second point in the further proof that motor ability may be meaningfully understood, namely, that it represents a group of abilities in the main, independent of, or at least different from mental ability."[2]

It is to be noted that the weightings derived from one giving of the test did not give the same values for the second giving of the test, and that these second values were quite moderate for multiple correlations, not over .65. Furthermore, the battery was made up by trying out a large number of tests and keeping only those which showed the higher correlations. When working with low and relatively insignificant correlations close to the usual limit of 3 P.E., this method does not seem to hold very well, since second givings of the test are quite likely to show a general lowering of intercorrelations of the selected tests, just as would happen if one selected coins on the basis of which came up heads most often and then tried them a second time.

[2]Cowdery has treated Garfiel's multiple correlations by a slightly different technique, adding some of the tests not included by her in the multiple, and discarding others. He obtained slightly higher values.

Furthermore the argument by analogy from the field of "general intelligence" is dangerous since it is open to the same objections. Also the tables of correlations usually cited as to homogeneity of "mental" tests are usually for very large ranges, which helps to raise intercorrelations. Since the motor tests are usually for small ranges of college students, the correlations are not strictly comparable.

The article by Hull (15) on the predictive value of tests seems to the writer to point out the futility of trying to "explain" small correlations, since the lower values of r are spuriously large in appearance because non-linear in their relationship to predictive efficiency as shown by the percentage of reduction of the standard error of estimate over a chance guess. The type of result which may be expected from multiple correlations of a number of small individual correlations is shown by those of two other studies, Patten (24) and Gleason (13), in predicting a complex performance, engine lathe work in a college engineering shop. Here the weightings derived from one year's giving of the tests against an objective criterion of measured work products did not come out the same in a repetition of the test a year later on another group in the same laboratory.

Before summarizing the evidence as to a general motor factor any further, it is well to consider the second hypothesis which also bears upon the first. The second hypothesis assumes that skill in complex performances is analyzable into speed or accuracy of such performances as simple reaction-time, reflexes, and measures of steadiness in thrusting and tracing with a stylus.

Analysis into Simple Actions. Hypothesis Number 2 may be called "analysis into simple actions and reflex components." Before investigating the analysis of any complex reaction in this way, it is advisable to consider the reliabilities and interrelations of various "simple reactions." Only one study, that of Farnsworth, Seashore, and Tinker (9), reports a reliability coefficient. On 34 university men and women $r_{\frac{1}{2}\frac{1}{2}}$ was .70±.06 for the first trials against the second 50 trials (not stepped up). This was for simple reaction of fingers on a telegraph key to a moving visual stimulus. On the interrelations of the simple reaction-times of different musculatures, Reymert (29) studied the reactions of 7 different portions of the body to the same type of auditory stimulus. The coefficients of correlation based on 20 university men and women ranged from $r = .42 \pm .13$ to .80±.06. A fairly representative coefficient would be from .55 to .60. His results are given in Table 2.

TABLE 2

INTERCORRELATIONS FOR SPEED OF SIMPLE REACTION TO AUDITORY STIMULUS (REYMERT, 29)

(20 university subjects, men and women)

	1	2	3	4	5	6	7	8	9	10	11
1		.39	.15	.50	.47	.10	.67	.33	.56	.60	.64
2			.78	.71	.63	.44	.55	.75	.49	.39	.07
3				.47	.45	.42	.53	.52	.43	.06	—.24
4					.66	.66	.71	.80	.66	.37	.04
5						.53	.66	.73	.48	.51	.46
6							.54	.62	.45	—.17	.12
7								.60	.76	.33	.35
8									.45	.15	.12
9										.33	.36
10											.48

P.E.'s range from .15 to .06

1. Tapping telegraph key 5 seconds
2. Simple reaction to auditory stimulus—teeth
3. " " " " " —lips
4. " " " " " —index finger
5. " " " " " —head
6. " " " " " —elbow
7. " " " " " —thumb
8. " " " " " —foot
9. Choice reactions to weaker of 2 sounds
10. Writing numbers
11. Counting numbers orally

In summary it may be said that the persistence of individual differences is only moderately high for the study of individuals, and on this account interpretations of simple reactions in relation to other simple and complex performances is quite uncertain.

As examples of attempts to relate speed of simple reaction on a telegraph key to speed in tapping a telegraph key for periods of 5 seconds, the following correlations may be considered:

r	*P.E.*	*N*	Investigator
.06	.10	51 university men and women	Perrin
.50	.12	20 " " " "	Reymert
.21	.05	157 " " " "	Ream
.10	.10	44 university men	R. H. Seashore

From this it can be seen that simple reaction of the usual laboratory experiment is not a measure of speed in general, since it shows insignificant correlations with another measure of speed for continuous performance on much the same type of instrument.

Neither is simple reaction in pressing a telegraph key indicative of speed in moving a short distance and pressing the key, as shown by the following correlations of Farmer and Chambers on two groups of young factory women, numbering 17 and 23, respectively: $r = .17 \pm .16$, and $r = .47 \pm .11$. For these same groups simple reaction in merely depressing a telegraph key showed correlations ranging from $-.06 \pm .16$ to $.41 \pm .12$ with speed of movement over a short distance to press the correct one of from 2 to 6 equidistant keys in discriminative reactions. However, the test of simple movement over short distance did correlate with the above-described discriminative movements as shown by coefficients ranging from $.20 \pm .16$ to $.83 \pm .05$, and a fairly representative coefficient for these tests on these groups would be between .60 and $.65 \pm .10$.

Perrin (26) reports for his same group of 51 students a correlation of $.59 \pm .06$, between simple reaction to color stimulus and discriminative reaction to color stimuli. (Shown in Table 1.)

Hansen (14) reports a correlation of $.29 \pm .05$ on 151 university students between simple reaction to auditory stimulus and speed of serial discriminative reaction on a typewriter in response to auditory stimuli. Farnsworth, Seashore, and Tinker (9), working with 34 university men and women, report a correlation of $.15 \pm .11$ between simple reaction to a moving visual stimulus and serial discriminative reaction to moving visual stimuli (numbers) on the Seashore serial discrimeter (4 keys, simple escapement mechanism which controls accuracy).

In considering the intercorrelations of "simple reaction-times" with other complex measures, the tables of correlations from the study of R. H. Seashore may be cited. In general it may be said that simple reaction-times show rather consistently low positive correlations with these more complex measures.

An examination of other relatively simple types of tests, such as steadiness in tracing a narrowing path with a stylus (Whipple, 39), or of thrusting a stylus at a series of various-sized holes, tends to show that these are not general measures of hand- or arm-steadiness, since they do not correlate with each other. In both tests, however, interpretation is very difficult on account of ambiguous scoring systems which are in turn partly due to the fact that rate of movement and similar factors are not well controlled. No reliabilities are reported for adults, but on a group of preschool and elementary

school children, 3-6 years of age, Wellman (38) reports a reliability of .90±.01 for retest.

TABLE 3

INTERCORRELATIONS OF SIMPLE AND SERIAL MOTOR TESTS (SEASHORE, 32)
(N=44, university men)

Test	R.T.	Mot.	T.P.	S.T.	Pur.	M.R. 40
M.R. 50*	.14	.28	.39	.32	.46	.64
M.R. 40	.31	.02	.38	.23	.41	
Pur.	.15	.40	.25	.31		
S.T.	.26	.37	.30			
T.P.	.33	.04				
Mot.	.10					

Reliability M.R. 40 Trial 1 *vs.* Trial 2 r—.80
By Brown's formula x 2 (actual length of test) r—.89

*M.R. 50	Seashore motor rhythm test (serial) at 50 R.P.M.
M.R. 40	Seashore motor rhythm test (serial) at 40 R.P.M.
Pur.	Koerth pursuit rotor
S.T.	Whipple steadiness tracing
T.P.	Whipple thrust precision
Mot.	Speed of tapping telegraph key for 5 seconds (Ream technique)
R.T.	Simple reaction-time (on telegraph key) to auditory stimulus

PREDICTIVE VALUE OF PEARSON COEFFICIENTS OF CORRELATION

r	$P.E.$	% reduction of $S.E._{est.}$
.00	.10	0.00
.10	.10	0.05
.20	.10	2.02
.30	.09	4.60
.40	.08	8.90
.50	.07	13.40
.60	.06	20.00
.70	.05	28.60
.80	.04	40.00
.86	.02	50.00
.90	.02	56.40
.95	.01	68.90

In contrast to the usual low intercorrelations shown by these "steadiness" tests, Spaeth and Dunham (35) report a correlation of .61±.106 between Dunlap's test for precision in thrusting a stylus into various-sized holes and scores of rifle target shooting of 73 army men ranging from poor to expert marksmen. This value is contrasted with the very low values found by Perrin (26), Garfiel (11), and Seashore (32) on similar tests of the type described in Whipple's *Manual* (39).

An evaluation of the second hypothesis, i.e., the analysis of complex motor performances into simple reaction-times, speed of reflex action, steadiness in arm movement, etc., must be only tentative on account of the extremely small amount of data on both reliabilities of measurements and intercorrelations with other simple and complex performances. At present it can be said only that there does not seem to be any general measure of even such a limited class as speed of hand movements. And even in cases where it would seem that similar stimuli and similar musculatures were involved, these simple measures are only slightly related to either continuous action of the same sort or to continuous reaction of a more complex type. In this connection it is of interest to consider the summary of Reymert from his study previously cited: "There seems to be a common factor (in the nature of a personal equation) operating within instantaneous motor capacities (reactions) as well as within continuous motor performances, also for constancy, but this personal equation (similarly to that of speed) is to some degree a different one for *instantaneous* than for *continuous motor performances.*"

To summarize the experimental findings of various sorts of simple motor performances:

1) Simple reaction-times of different musculatures to the same stimulus show a moderately high intercorrelation of about .60 (Reymert, 20 women). The reliabilities of such measurements are but little known, but Farnsworth, Seashore, and Tinker report an $r = .70 \pm .06$ for the first 50 against second 50 trials of simple reaction to a moving visual stimulus. Simple reaction-time in merely pressing a telegraph key does not correlate with simple reaction in moving a short distance before pressing the key (Farmer and Chambers).

2) Simple reaction-time does not correlate with speed of tapping a telegraph key for short periods (Perrin, Reymert, Ream, Seashore). Simple reaction to colors correlated only moderately with simple discriminative reaction to colors (Perrin), and speed of simple movement over a short distance to press a key correlated moderately with discriminative reactions under similar conditions (Farmer and Chambers).

3) Simple reaction does not correlate with speed of serial discrimination to four stimuli in sound (Hansen) nor in the case of

moving visual stimuli (Farnsworth, Seashore, and Tinker). Neither did it correlate with accuracy in Koerth Pursuit test or Motor Rhythm, which are serial measures (Seashore), nor has it shown any correlation with other diverse motor performances, whether single or serial performances.

To judge from the consistently low positive intercorrelations of motor performances we are not justified in speaking of a general motor ability since persons may rank consistently high in one field, e.g., serial discrimination, and consistently low in other motor fields, e.g., pursuit tests. Neither are the complex motor performances analyzable into such units or variables as simple reaction-time, steadiness of hand movement, or other simple components.

Basic Motor Capacities. A third hypothesis has been proposed in which complex motor performances are considered to be combinations of a small number of *basic motor capacities,* which are serial in nature. These basic motor capacities are considered to be largely a matter of endowment, only slightly improvable by practice,[3] and basic to more complex performances.[4] Among the activities which have been listed in this group are motility or speed of tapping, serial discrimination, precision in motor rhythm, pursuit or eye-hand coordinations, and postural stability. The studies of Hansen on serial discriminative finger movements on typewriter keys to four different stimuli showed that serial discrimination to spaced sound stimuli correlated only .48±.11 with the same type of movement in response to four different typewritten letters. Hence one must either say that sensory factors are very important in determining the measurements of such motor coordinations or that there are at least two basic motor capacities of serial discrimination, with the probability that other sense fields would show still other basic motor capacities for each field tested and perhaps for different types of stimuli in the same sensory field. The studies of Hansen (14) and also of Robinson reported insignificant correlations with speed of typing in classes. Some of the principle correlations of Hansen's study are reproduced in Table 4.

As a result of work in the same laboratory, following Hansen, the writer became impressed with the possibilities of the basic motor

[3]More recent experiments tend to show that practice results are greater than at first believed.

[4]Experimental results so far are uncertain, principally on account of uncertain criterion measures.

TABLE 4

INTERCORRELATIONS, SIMPLE AND SERIAL MOTOR TESTS (HANSEN, 14)

Simple reaction to sound *vs.* serial discrimination to sound					
r	.29	*P.E.*	.05	*N*	151
r	.46	*P.E.*	.09	*N*	25
Simple auditory discrimination reaction *vs.* serial auditory discrimination					
r	.35	*P.E.*	.05	*N*	150
r	.37	*P.E.*	.10	*N*	26
r	.37	*P.E.*	.10	*N*	26
Tapping *vs.* serial discrimination					
r	.25	*P.E.*	.05	*N*	51
r	.24	*P.E.*	.10	*N*	43
r	.27	*P.E.*	.07	*N*	61
r	.23	*P.E.*	.05	*N*	174

RELIABILITIES OF SERIAL ACTION

First day *vs.* mean of all 5 days						
r	.88	*P.E.*	.03	*N*	26	
r	.89	*P.E.*	.03	*N*	26	(auditory)
First day *vs.* last day						
r	.76	*P.E.*	.07	*N*	26	(visual)
r	.63	*P.E.*	.09	*N*	26	(auditory)
Serial discrimination; visual *vs.* auditory stimuli						
r	.48	*P.E.*	.11	*N*	26	(first day)
r	.52	*P.E.*	.11			(all 5 days)

capacities hypothesis, and along with other investigators was greatly puzzled by the low predictive value of his test of serial action. The result was the building of a new apparatus for serial discrimination called the Serial Discrimeter (33) in which the writer worked from Hansen's model and was aided by the members of the Iowa Laboratory. Two possible defects of Hansen's apparatus were considered to be possible causes of its low predictive value; first, the fact that when measuring speed, accuracy could not be controlled other than by direction to go at a rate sufficiently high that about 5 mistakes per line would be made; and, secondly, the typewriter mechanism may have been slow enough in action as to constitute a large part of the total time taken to complete a series, thus tending to obscure the individual's speed. In addition, the usual criterion difficulties of having arbitrary accuracy weightings in a speed score in typewriting were considered possible causes of low predictive findings.

The general aim of the writer's research has been to investigate performances on serial motor tests which have objective scoring in terms of a single integrated score. This is made possible by mechanical controlling of variables other than the one measured.

Individual Differences Due to Training. A fourth hypothesis, perhaps the most difficult to evaluate, is the ascription of individual differences in various motor tests to differences in previous training, both direct and transferred, and in all degrees of efficacy. This was touched upon in this study by the two minor studies upon ratings of training in three fields which were apparently related, and by an examination of the scores made by very highly skilled athletes and instrumental musicians.

Very little has been known concerning the intercorrelations, reliabilities, and predictive efficiencies of various measures of serial action, where measurement is in terms of a single integrated score and other variables are controlled by the mechanism. Since many of the mechanisms are rather bulky, it was also desired to know which should be adapted for use in a portable unit outside of the laboratory for actual vocational or personnel work, and to find out the approximate relations which might be expected with skill in other more complex fields which seemed to be apparently related. Previous work had offered very little data of this sort on which to base theoretical and job analyses of motor skills.

In the absence of a stable theoretical basis of motor skills, and considering the lack of knowledge as to the persistence of individual differences in such skills, the writer has considered it advisable to make an intensive laboratory investigation of the basis of personnel work in this field before attempting to apply the techniques in practical skills.

The Experiment

This research reports data on eight representative types of motor performances ranging from grosser to finer coordinations, and including those having dominantly visual, auditory, and kinaesthetic cues. All are serial in nature and are measured objectively in terms of single integrated scores. The tests and the types of performance are as follows:

1) Miles Ataxiameter, amount of bodily sway in erect position, without visual control
2) Miles Pursuitmeter, accuracy in following an irregular series of movements of a wattmeter indicator by compensating movements of a rheostat lever
3) Koerth Pursuit Rotor, accuracy in following a small target revolved by a phonograph in a circular path at a high rate of speed

4) Serial Discrimeter, speed in making discriminative finger reactions to number signals appearing as fast as reacted to correctly
5) Motor Rhythm Synchrometer, temporal precision in following a simple repeated auditory rhythm on a telegraph key
6) Miles Pursuit Pendulum, accuracy in following a double swing of a pendulum, catching water emitted from the pendulum, using small tubes
7) Miles Speed Rotor, speed of rotary arm, wrist, and finger movements in turning a small hand drill having a Veeder counter
8) Brown Spool Packer, speed of a bimanual coordination resembling an industrial operation

All work was done individually under the same experimenter. Knowledge of results was available to observers at all times from the automatic scoring of the performances, with the exception of the pursuit pendulum, where only visual estimates were available until the exact scores could be measured before the beginning of the second half of the experiment.

The tests were given under the Miles cycle plan, whereby about 5 minutes work was done on each test in rotation. The cycle of 8 tests took 40 minutes, with 3 cycles given the first day. Forty-eight hours later 3 more cycles were done. Having also done work by the method of completing each test before going to the next, the writer believes that the cycle method reduces fatigue and keeps up interest for both experimenter and observer by the rapid shifting from one task to another. In addition, there are some grounds for believing that spaced learning operates to a greater extent, and that transfer of training from one activity to another is more equalized, even though it is probably small.

The observers were 50 Stanford University men, about 35 of whom were undergraduate volunteers, and 15 of whom were graduate students drawn from a class in physiological psychology of action. All observers had previously seen most of the tests (all of the apparatus which was portable) demonstrated in class. In addition each observer received a printed sheet giving general information about the tests, the type of performance involved in each test, and general instructions as to the necessity for working at maximum efficiency in shifting from one type of activity to another. All tests were first explained by standardized instructions with demonstrations. Each performance was demonstrated in outline

by the observer in order to see that he knew the objective. He was allowed to ask questions at any time. The instructions always included suggestions as to the best ways of doing each test, as reported from previous work. The attitude and performance of the observers were very satisfactory in maintaining constant attempts at improvement throughout the rather lengthy series.

Wherever possible, the instructions were taken practically verbatim from the original descriptions of the apparatus. These need no further description other than to say that they were as complete as possible in order to minimize problem-solving aspects and emphasize skill in actual performance.[5]

Experimental Results. The data of this experiment have been treated by tabulation of the averages of each individual's scores on each test for the first day and also for the second day. The average scores of the first and second day's work on each test were correlated by the Pearson product moment method in order to obtain measures of reliability, or persistence of individual differences through this short period of training. These results were as follows:

Test	$r_{\text{I-II}}$	*P.E.*
Miles ataxiameter	.86	.03
Miles pursuitmeter	.81	.04
Koerth pursuit rotor	.75	.05
Serial discrimeter	.94	.02
Motor rhythm synchrometer	.82	.04
Miles pursuit pendulum	.80	.04
Miles speed rotor	.88	.03
Brown spool packer	.86	.03

These measures (not stepped up), ranging from .75 to .94, have an average value of .84, which would seem to be sufficiently high for use in studying individual differences. These reliabilities would not account for any great lowering of intercorrelations of the different tests due to attenuation.

For the study of interrelations of the different tests, the averages of the second day's performances were used, since results on other tests (9, 16) have usually shown that successive samplings of performances increase in reliability and to some extent in their in-

[5]For description of most of these standardized instructions and techniques see Seashore "Stanford Motor Skills Unit (34)."

tercorrelations with other performances measured under similar conditions.

Table 6 gives the intercorrelations of all second day scores, and in addition the correlation of each test with the seven other tests in the battery when combined with equal weight. This latter type of correlation gives values only slightly higher than the average intercorrelation of that test with each of the others and shows the extent of relationship of each test with the others without weighting.

Since the number of cases is the same for all, a table of *P.E.*'s is given, together with the attendant reduction in standard error of estimate, which may be called a predictive index. An inspection of this table shows only one negative intercorrelation, that

TABLE 5

DISTRIBUTION OF TRIALS, CYCLES, AND SCORES

Test	Scoring unit	Length of trial	Trials per cycle	Maximum possible score
Atax.	total sway, mm. per min.	1 min.	2	unlimited
Pm.	arbitrary elec. units	5 min.	1	unlimited
K.P.R.	counter units	20 rev. @ 60 r. p. m.	5	200 per trial 1000 per cycle
S.D.	correct reactions	2 min.	2	unlimited
M.R.S.	correct reactions within .05 sec.	1 min. @ 50 r. p. m.	3	200 per trial
P.P.	cc. water per double swing	25 tubes	1	1250 cc. per trial
S.R.	counter units crank rev. × 4.3	10 sec.	5	unlimited
B.S.P.	spools packed	3 min.	1	unlimited

AVERAGES SCORES OF TEN REPRESENTATIVE OBSERVERS

Test	First day Cycles I	II	III	Second day Cycles IV	V	VI
Atax.	685	671	639	690	628	594
Pm.	893	790	745	678	615	595
K.P.R.	171	466	550	553	690	734
S.D.	181	200	204	202	215	218
M.R.S.	144	147	159	152	148	156
P.P.	512	688	765	741	809	849
S.R.	224	236	237	241	246	248
B.S.P.	167	183	185	187	193	200

TABLE 6

INTERCORRELATIONS OF MOTOR SKILLS TESTS

(Average of second day's scores)*

Test	Atax.	Pm.	K.P.R.	S.D.	M.R.S.	P.P.	S.R.	B.S.P.
Ataximeter		.19	.12	—.15	.03	.16	.12	.15
Pursuitmeter	.19		.29	.18	.17	.14	.09	.26
Koerth pursuit rotor	.12	.29		.25	.40	.56	.33	.26
Serial discrimeter	—.15	.18	.25		.29	.33	.08	.32
Motor rhythm synchrometer	.03	.17	.40	.29		.36	.63	.43
Pursuit pendulum	.16	.14	.56	.33	.36		.23	.44
Speed rotor	.12	.09	.33	.08	.63	.23		.38
Spool packer	.15	.26	.26	.32	.43	.44	.38	

CORRESPONDING VALUES OF r AND *P.E.* WHEN $N=50$

r	*P.E.*	r	*P.E.*
.10	.09	.45	.08
.20	.09	.50	.07
.30	.09	.55	.07
.35	.08	.60	.06
.40	.08	.65	.06

Test	Range	*S.D.*	Mean
Ataxiameter	240-738	105.3	392.5
Pursuitmeter	378-1134	163.4	642.9
Koerth pursuit rotor	395-839	106.1	642.5
Serial discrimeter	164-311	28.8	226.3
Motor rhythm synchrometer	57-196	30.3	157.1
Pursuit pendulum	582-928	95.0	778.7
Speed rotor	192-287	19.3	245.9
Brown spool packer	190-282	21.8	233.9

*All scores have been arranged to compare best scores, since a good score on the pursuitmeter, for instance, is a low error record, whereas a good score in spool packing is high output.

between serial discrimination and ataxiameter which is —.15. The highest intercorrelation is .63, between motor rhythm and speed rotor. A representative intercorrelation is about .25, which confirms the general findings of other studies in showing a slight positive intercorrelation between most motor tests (Garfiel, 11). It is to be noted that the three pursuit tests are correlated only very moderately, and others, still less. In view of the non-linear progressive values of r, it is of particular interest to note the cor-

responding values of reduction of standard error of estimate which are indicated by the values of r (see p. 45).

Special Experiments

The relation of differences in motor skills to previous training has been investigated in two ways. In the first, the 50 observers of the main experiment filled out printed forms summarizing the amount of training[6] in the three fields of athletics, instrumental music, and typing, and any other special training which resembled these activities. These blanks were then rated into five classes for amount of training in each separate field. Ratings in each field were made by two persons well acquainted with that field. Each rater received a description of the five steps to be used. When necessary half steps were used. The reliability of the independent ratings for separate fields, made without knowing the names of the observers, and judging only from the single fields of their specialties, ranged from .93 to .97±.01. It is realized, however, that the observers' own descriptions of their training were probably not as reliable as the rating of their descriptions. The training rating used for each field was the average rating of the two judges. Table 7 gives intercorrelations between training rating and the score on the battery of motor tests, equally weighted by means of standard scores, and with the few separate motor tests which showed any evidence of relationship from the correlation plots. Those which are omitted were undoubtedly within a few points of a zero correlation.

TABLE 7

Intercorrelations of Training Ratings and Motor Skills Tests

			r
Typing	*vs.*	Battery	.03
		Motor rhythm	.12
		Serial action	—.14
Music	*vs.*	Battery	.40
		Motor rhythm	.37
		Serial action	.57
Athletics	*vs.*	Battery	.03
		Koerth pursuit rotor	.41
		Pursuit pendulum	.20
Combined	*vs.*	Battery	.30

[6]Such variable amounts of training may not be very closely related to their proficiency in these lines. This part of the experiment is only a suggestion of what needs to be done on a larger scale.

The second minor study of the relation of training to these motor skills consisted of a comparison of the scores of small groups of highly selected athletes and instrumental musicians with the larger unselected group of 50 men in the main experiment. These special observers were selected by specialists in their respective fields as being among the most highly skilled in their lines in the university.[7]

The small numbers in the two groups seriously limit any interpretation which may be placed upon their results, but this is counterbalanced by the fact that they were very highly skilled in their respective lines, and further, that the results of the two groups are consistently borne out by the individual members. No person of the highly skilled groups ranked below the average score of the unselected group for the whole battery of laboratory tests. None of the scores of these special groups on individual tests is greatly below the average for the unselected group, and most of the scores are either slightly above average or considerably so. Table 8 shows the standard scores of the highly skilled individuals, and the portion of a standard deviation on each test and the battery as a whole by which they are separated from the unselected group.

It will be observed that the musicians excelled on two tests, the serial discrimeter and motor rhythm synchrometer, and were only slightly above average on the rest. Their excellence on these two tests, however, ranked them approximately .5 standard deviation above the unselected group on the battery as a whole.

The athletes tended to excel on two tests, the Koerth pursuit rotor and the Miles pursuit pendulum, and were close to average on the remaining tests, being slightly higher than the musicians, however. The average of their scores on the battery as a whole was approximately .8 standard deviation higher than the average of the unselected group.

Motor Skills and Verbal Tests

Table 9 gives the intercorrelations of the Thorndike College Entrance Test with the separate motor tests and the motor battery as a whole. It will be noted that none of these correlations is very

[7]The writer is greatly indebted to Mr. Warren D. Allen, university organist, for aid in securing the musicians, and to Messrs. C. W. and H. Davis of the Physical Education Department for aid in securing athletes.

TABLE 8

SECOND DAY'S SCORES OF HIGHLY SKILLED MUSICIANS AND ATHLETES
(Standard deviations above or below mean of unselected group)

		Ataxiameter	Pursuitmeter	Koerth pursuit rotor	Serial discrimeter	Motor rhythm synchrometer	Pursuit pendulum	Speed rotor	Spool packer	Battery
Pianists										
	B	.9	.9	—1.5	2.4	1.2	—.5	1.4	.8	1.1
	SSS	1.4	—.1	.2	.0	1.2	.4	—2.5	—.3	.0
	Gr	—1.7	1.1	—1.	2.	1.2	.0	—1.	.1	.1
	Ga	.4	—.1	—.3	1.7	.6	—.5	.6	.6	.6
	Av.	.5	.4	—.6	1.7	1.1	—.2	—.4	.3	.5
Athletes										
	Be	1.4	1.6	.9	—1.1	—.4	.0	.1	—1.2	.2
	H	1.6	.9	2.1	1.	.6	1.6	—.2	.4	1.6
	Bl	—1.	.4	.7	.3	1.3	.4	2.1	1.5	1.1
	W	1.2	1.6	1.6	—.5	.9	1.	.3	—.3	1.5
	Hu	.4	.4	1.1	—.7	—1.1	.6	—.2	.4	.2
	Ha	.2	.1	1.1	.0	.6	1.	—.7	.4	.3
	Av.	.6	.8	1.2	.0	.3	.8	.2	.2	.8
Retest observers*										
	Hi	.0	2.6	—.3	.7	.9	1.2	.8	—.1	
	P	.9	2.4	.9	1.7	.2	2.1	—.7	1.3	
	L	1.9	2.6	1.8	.3	—1.7	2.5	.6	.1	
	C	—1.	1.1	.7	1.7	.2	2.7	.1	1.3	
	He	.4	1.1	1.6	.0	—.1	2.7	1.4	.4	
	Pe	—.5	2.4	.2	1.4	.9	2.3	—.7	—1.	
	Av.	.3	2.	.8	1.	.1	2.2	.2	.3	

*Observers trained intensively one year previous on ataxiameter, pursuit meter, and pursuit pendulum.

significant. This agrees with the general summary of previous researches cited by Garfiel (11); also with the study of Farnsworth, Seashore, and Tinker (9) which reports correlations with the serial discrimeter and Thorndike tests; and with Seashore's work on motor rhythm at Iowa in which a correlation based on 37 of his observers (university men) showed similar results: $r = .06 \pm .12$ between the Koerth pursuit rotor and Thorndike scores.

INTERPRETATION OF RESULTS

A general summary of the intercorrelations of the motor skills tests employed in this research shows that the different types of

TABLE 9

INTERCORRELATIONS OF MOTOR SKILLS BATTERY WITH SINGLE MOTOR TESTS, AND THORNDIKE TEST SCORES (N=50)

Test	Motor skills battery*	Thorndike
Miles ataxiameter†	.11	.05
Miles pursuitmeter	.23	.23
Koerth pursuit rotor	.40	—.15
Serial discrimeter	.23	.09
Motor rhythm	.42	—.15
Miles pursuit pendulum	.40	—.31
Miles speed rotor	.33	—.33
Brown spool packer	.40	—.03
Motor skills battery		—.14

*In this treatment the average of the second day's scores on each test is correlated with the remaining 7 tests, equally weighted by the following formula:

$$r = \frac{r_{01}\sigma_1 + r_{02} + \ldots\ldots + r_{0n}\sigma_n}{\sqrt{\sigma_1^2 + \sigma_2^2 + \ldots + \sigma_n^2 + 2(r_{1n}\sigma_1\sigma_n + \ldots + r_{1-n,n}\sigma_{n-1}\sigma_n)}}$$

The author is indebted to Mr. Harold A. Edgerton for this treatment of the data.

†Correlations of ataxiameter with height —.33; and with weight, —.09.

performance are but slightly related, even in those cases in which a priori estimates might have expected interrelationship. The average intercorrelation of .25±.09 agrees with practically all of the other data previously reported in showing a very slight positive relationship. The slight extent of this relationship is still better seen when the values of r are expressed in terms of the reduction of standard error of estimate over chance guesses. Furthermore, these measures were serial in nature, as are actual skills of everyday life, and the reliability of their single integrated scores shows that the intercorrelations could not have been greatly affected by attenuation. Since the intercorrelations are based upon only the last half (2 hours) of the tests, they might have been expected to afford a slight raise in intercorrelations due to practice effects. The phenomena of correlated errors, such as the fact that each test was done on the same day under much the same conditions of health and influences of preceding events, might be expected to add a small amount of correlation, even if other factors tended to make it zero.

A comparison of the cycle method of testing with results under other conditions tends to show the same general results as to interrelations of the tests. (Cf. Seashore, 32.)

We are now able to evaluate one point in the method of theoretical and job analyses of skills, namely, the validity of the term "motor ability." Judging it purely by its usefulness, it might be urged that if a person scores high on a large and representative number of motor performances, it would be permissible to say that that person has shown "motor ability." Conversely, it might be said that if he failed in a large number of such performances he lacked "motor ability." Two major objections may be made, however: (1) A person may rank consistently high on one or several performances, and consistently low on several others, so that an average score would merely tend to obscure any individual differences which might possibly be used for predictive purposes; and (2) if a person is said to possess motor ability, as judged from one or several tests, e. g., a pursuit test, he may be no better than average on another test, and outside factors must be called in.

As a test case, the Brown spool packer was included in our battery as one to be predicted by multiple correlation from the other measures. Notwithstanding the reliabilities of the tests, this multiple coefficient reached a value of only $.57 \pm .11$. An inspection of the other tests would seem to indicate that perhaps one of them could have been better predicted by a multiple correlation, but even here the same objection would hold that giving the tests a second time would not be at all certain to show the highest of the moderate correlations with the same test.

In consideration of the results on simple and complex motor tests, the following statement seems to the writer to be the most conservative evaluation of the principles involved in personnel treatment of individual differences in motor performances: Any motor test is a *sampling* of behavior measured on the *assumption* that this series of measurements will represent in some degree what the individual will do on similar performances later on, with the *further possible assumptions* that (1) (aptitude) the measurement is not a "miniature performance" of the skill itself, but some more basic coordination common to this and many other complex skills; or (2) (miniature performance) that the measurement is a sampling of more or less independent skills, on the theory that if the person has acquired these samples it is probable that his antecedents have been such that he has acquired others to about the same degree.

This statement does not depend on the specific or general nature

of motor skills, nor on the relative influence of nature or nurture, nor does it claim that the same type of results should be expected from testing different ages and ranges of individuals. As such it is only a formulation of the general bases for the prediction of motor skills, and, to be useful, certain modifications must be made.

These qualifications are in answer to the questions listed in the introductory statements of the problem. What type of units or variables may be used in theoretical and job analyses of skill? On the populations of college students which have been tested, the interrelations of individual differences on motor tests do not seem to justify the assumption of a general motor ability. Since an individual may rate high in certain kinds of skill and low in others, prediction for any complex skill should supposedly be made in terms of simpler skills involving the same sensory fields and the same general musculatures, as suggested by Muscio. However, it is yet to be demonstrated that even this type of predictive analysis is practical, or that it will hold consistently. We can, however, point out that such units or variables as speed of hand movement, or steadiness of hand movement, etc., do not appear to be justified by the experimental findings, although it may be possible that speed of hand movements *may be* related in *very limited* and homogeneous fields of action, as suggested by the correlations between simple and discrimination reactions to the same type of stimuli.

We may also say that, within the limits of experimental errors, of inequalities in previous training in not starting at the same point, and in employing different methods of work, there is very little evidence for any "general learning ability." This is shown by the low intercorrelations between the different serial coordinations which were used in this and other experiments.

As to the validity which may be expected of these motor tests, the following points may be considered: The persistence of individual differences in these skills as shown by test reliabilities of the serial performances is sufficient for use in predictive technique. The few experimental findings as to predictive value of these is so low that it may reasonably be expected to show validity in predicting only the upper and lower extremes of distributions. This must be further qualified in two ways: first, that the prediction is likely to work only on complex skills which are dominantly motor and in which other factors are not of major importance or are

controlled; and, secondly, that the prediction is likely to be of use only where the criterion measurement can not be easily influenced by relatively short periods of training such that most people could easily qualify with training. The intercorrelations of the tests show that they have a slight overlapping, that they are relatively independent skills. From the statistical point of view this is considered desirable in assembling a battery of tests. It furthermore fits in with the hypothesis of measuring samplings of motor skills to predict achievement in other fields. The principal factor limiting the usefulness of any battery of tests is the relative importance of present ability or achievement to the special "techniques" or work methods involved in the skill to be predicted. The importance of technique will vary greatly from one skill to another, and the writer is undertaking an investigation of its importance in the tests themselves. This point is critical to many tests, not only to motor performances, and may affect even the simplest acts.

The third and fourth aspects of this research have been the determination of the lines of research in motor skills which are likely to be of most importance in the near future and the techniques for their investigation. In addition to studies analyzing "techniques" or methods of work in relation to measured individual differences, two other lines of research stand out. First is the tryout of serial tests in actual working conditions, to predict athletic, instrumental musical, and industrial performances. It is necessary that the serial tests be measured in terms of a single integrated score to avoid ambiguity and unreliability of scores. Likewise the criterion measurement must be in terms of a single integrated (not arbitrarily weighted) score. For use outside the laboratory a simplified portable unit involving six of these serial tests has been devised (34) and has been described for publication.[8]

The second line of attack is the investigation of implicit neuromuscular action by the action current technique worked out by Travis and others on the knee-jerk (37). Critical points would

[8]A motion picture film demonstrating the use of this unit has been prepared for the standard small size motion picture projector, and may be secured through the Stanford Laboratory. The description of apparatus gives working drawings, photographs, and instructions for operation. It is entirely possible that different age and economic ranges of populations studied would give results which differed consistently from the school and college ranges reported so far.

be the intercorrelations of action current times of reflexes, and their relations with serial overt performances.

A final qualification of the use of serial tests should be that, if reliable serial tests correctly used do not predict accurately measured criteria, we should then consider the possibility that individual differences may be a result of methods of work used by different persons and by the same person at different stages of learning, and that the most likely procedure for securing greater skill is to investigate the methods of work of the best workers and the most efficient methods of training in those techniques.

Résumé

1) *The Experiment.* Fifty men were tested on 8 serial motor performances, the Miles ataxiameter, pursuitmeter, pursuit pendulum, and speed rotor, the Koerth pursuit rotor, the Brown spool packer, and tests of serial visual discrimination and motor rhythm. Tests were given by the Miles cycle plan, each cycle consisting of approximately 5 minutes' work on each test in succession. A cycle required 40 minutes. Three cycles were given the first day, and 3 more cycles 48 hours later. Work was arranged under conditions of full instruction and demonstration of methods to minimize difficulties of comprehension and emphasize skill in actual performance. Knowledge of results was available throughout the experiments.

2) *Principal Results.* Reliabilities of the tests (average of first day's scores *vs.* average of second day's scores) ranged from .75 to .94, averaging .84 (uncorrected). Intercorrelations of tests (average of second day's scores) ranged from —.15 to +.63, averaging +25. Apparently individual differences are specific in nature, but fairly persistent over this period of training.

3) *Special Results.* A few highly selected athletes, from all sports, excelled, on the average, on two pursuit tests and were average or slightly above on the other tests. A few highly selected pianists excelled, on the average, on the serial discrimination and motor rhythm and were about average on the remaining tests.

A few persons highly trained one year previous on the Miles ataxiameter, pursuitmeter, and pursuit pendulum were given the entire battery including these three tests one year later. Skill remained at a high level in the pursuit tests but not in the ataxiameter.

Intercorrelations of the separate motor tests with the Thorndike

College Entrance Tests ranged from —.33 to +.23. Correlation of the Thorndike with the battery of motor tests equally weighted gave a coefficient of —.14.

4) *Interpretations of Nature of Individual Differences in Motor Skills.* The independence of the skills measured in these tests argues against any theory of general motor ability and in favor of specific skills. The results of related studies have so far presented little evidence that serial performances may be analyzed into speed or accuracy of simple reactions or reflexes.[9]

The theory by which motor skills are determined by a relatively small number of basic motor capacities is strongly open to question. The independence of these performances as measured suggests that, if there are basic motor capacities, they are more numerous and more specific than previously believed. Highly selected athletes were found to rank above average on the battery as a whole and significantly above average on a few separate tests. Ratings on amounts of previous training in athletics, instrumental music, typing, and other motor skills tend to show that these types of training do not have very great carry-over effects to the performances tested by this battery, but that there may be certain rather specific transfer effects.

Previous attempts to predict practical skills from serial motor tests have been somewhat inconclusive for lack of single integrated scoring systems, and the short and unequal periods of training may also have obscured relationships. A retest of this prediction technique has been facilitated by the development of a portable unit for the testing of motor skills, with standardized constructions, operation, and instructions for handling the tests. Thurstone's method of "critical level" might be applied as well as the usual correlation technique.

The influence of "technique," or methods of performing tests, may account for some of the observed individual differences in performances. That is, a person may score high, not only as a result

[9]The action current technique opened up by the work of Travis on the relation of "intelligence" test scores to speed of nerve conduction in patellar reflex must be applied to the field of motor skills also. Since it measures only the sensory and nervous portions of the reflex, leaving out the muscular activity which makes up so large a part of the reflex time as ordinarily measured, this type of analysis might perhaps show some general factor such as speed of nerve conduction. This would hardly seem likely in view of present results of very low correlations of motor performances among themselves and with "intelligence" test scores.

of "native abilities" or previous training, but also because of the particular way in which he went about the test. This is in harmony with Perrin's analysis of card-sorting (26); and it is also consistent with the independence and persistence of motor skills as found in this research. It is probable that achievement in these tests is determined by a large number of factors rather than by any one or two focal variables.

In consideration of the unstable basis of theoretical analyses of motor skills, the units or variables of job analyses likewise are apt to be unstable. As a most conservative procedure, it is suggested that a *sampling* of serial performances of the same general neuromuscular coordinations involved would be the most likely device for predicting a practical motor skill. Such a procedure is outlined in a description of a portable unit of motor tests.

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LES DIFFÉRENCES INDIVIDUELLES DANS LES HABILETÉS MOTRICES

(Résumé)

Cinquante étudiants universitaires ont subi des épreuves motrices *en séries* y compris des coördinations des doigts, des mains, des bras et du corps. On a évalué chaque test automatiquement pour un variable, contrôlant mécaniquement les autres variables. Les instructions complètes, les démonstrations, et la connaissance immédiate des résultats ont tendu à appuyer sur l'habileté dans les épreuves motrices plutôt que la resolution de problèmes.

Les intercorrélations peu élevées, environ 25, des résultats de ces épreuves motrices et d'autres plus simples indiquent chez les hommes étudiés que ces habiletés sont *spécifiques* et non déterminées par une *habileté motrice générale.* Des corrélations d'environ, 84 entre des périodes de deux heures pour chacun de deux jours montrent que les différences individuelles dans ces habiletés spécifiques sont très stables.

Les évaluations pour l'entraînement dactylographique, l'entraînement dans la musique instrumentale et la concurrence athlétique n'ont montré que de petites corrélations positives avec les tests moteurs. Cependant des groupes spéciaux de pianistes et d'athlètes *avec une très grande habileté* ont donné des résultats beaucoup plus élevés dans plusieurs épreuves. Les épreuves motrices ne montrent que de petites corrélations négatives avec le Test d'Intelligence Thorndike.

La vitesse dans les réactions *simples,* les réflexes, le caractère ferme des mouvements des mains, etc., ne nous permettent pas de prédire l'habileté dans les actes complexes, possiblement à càuse de l'importance de l'empiètement des mouvements. Certaines coöordinations *en séries* ont été appelées des *capacités motrices fondamentales,* données à entendre comme des avantages de naissance, lesquels ne peuvent être améliorés et possèdent une importance fondamentale pour les habiletés plus complexes. Cette expérience a trouvé une amélioration modérée et une haute spécificité. Apparemment il y a grand nombre de telles capacités ou, s'il n'y en a que peu, celles mesurées ne sont pas nécessaires comme bases aux coördinations pareilles. La spécificité des tests peut être aussi due au nombre de moyens de réussir dans chaque test, ce qui augmente aussi la variation des différences individuelles.

Le moyen le plus efficace de prédire une habileté motrice pratique semblerait être un "sampling" des épreuves *en séries* qui rèssemblent au travail. Une batterie portative de tests moteurs a été construite et sera décrite dans un article à suivre.

SEASHORE

INDIVIDUELLE UNTERSCHIEDE IN MOTORISCHER GESCHICKLICHKEIT

(Referat)

Fünfzig Männer der Universität wurden mit acht Serien motorischer Leistungen einschliesslich Hand, Arm, und Körperkoordinationen geprüft. Jeder Test wird automatisch für eine Variable bewertet, die andern Variablen mechanisch kontrolierend. Eingehende Anleitung, Demonstrationen, sofortige Kenntnis der Leistungswerte, erzielten vielmehr Geschick in motorischen Leistungen als die Lösung eines Problems.

Die niedrigen Interkorrelationen, ungefähr .25, der Leistungswerte dieser und anderer noch einfacherer motorischer Tests deuten an, dass diese Geschicklichkeiten bei diesen Vpn. *spezifisch* sind und nicht durch irgend eine *allgemeine motorische Fähigkeit* bestimmt werdem. Korrelationen von ungefähr .84 zwischen zweistündigen Perioden an je zwei Tagen erwiesen dass individuelle Unterschiede in diesen spezifischen Geschicklichkeiten sich ziemlich gleich blieben.

Leistungswerte der Geschicklichkeit auf der Schreibmaschine, im Spielen von Instrumenten und athletischen Übungen (zum Wettbewerb) erwiesen nur geringe Korrelationen mit den motorischen Tests. Doch besondere Gruppen *höchst geschickter* Pianisten und Athleten erhielten in verschiedenen Tests bedeutend höhere Leistungswerte. Die motorischen Tests zeigen eine geringe negative Korrelation mit den Thorndikeschen Intelligenz Tests.

Geschicklichkeit in komplizierten Handlungen lässt sich nicht von der Schnelligkeit in *einfachen* Reaktionen, Reflexen, Sicherheit der Handbewegungen, etc., vorausbestimmen vielleicht wegen der Wichtigkeit des Übereinandergreifens der Bewegungen. Gewisse *Serien* von Koordinationen hat man die *fundamentalen motorischen Fähigkeiten* gennant, i.e., angeborne Fähigkeiten verhältnismässig unverbesserlich und doch von höchster Bedeutung bei verwikelteren Geschicklichkeiten. Dieses Experiment bewies eine mässige Verbesserung und hohe Spezialisierung. Scheinbar gibt es entweder eine grosse Anzahl solcher Fähigkeiten oder wenn nur wenige, so sind die hier gemessenen nicht fundamental für ähnliche Koordinationen. Spezialisierung der Tests mag auch bedingt werden durch die verschiedene Art und Weise auf die man in jedem Test zum Ziel gelangt, wobei auch die individuellen Unterschiede zunehmen.

Wohl das zweckmässigste Mittel zur Vorherbestimmung motorischer Geschicklichkeit wäre eine Probe der Ausführung von einer *Serie* von Handlungen die der Arbeit ähnlich sind. Man hat eine tragbare Batterie von motorischen Tests hergestellt die in einem späteren Artikel beschrieben werden.

SEASHORE

AN EXPERIMENTAL STUDY OF THE RELATIONSHIP BETWEEN SPEED AND MENTAL ABILITY*

From the Psychological Laboratory of Harvard University

Ross A. McFarland

Statement of the Problem

In spite of the many shortcomings of the mental tests, everyone recognizes that certain abilities can be indicated and that others can be measured to a high degree of constancy. The systematic psychology behind intelligence, however, is quite a barren field. There is a great deal of confusion[1] as to the nature of intelligence, or, in other words, as to what the mental test material really tests. Opinion differs as to whether speed, accuracy, memory, attention, insight, or what-not is inherent in the mental reaction. While the time factor in human reactions has been approached from many points of view, its relation to mental ability in general is by no means clear. The evidence presented in a review of the literature (15) leads one to feel justified in stating that the ability to be quick is a very essential part, if not one of the crucial factors, in the mental reaction. It may eventually be found by experimentation that a number of factors are combined in the essence of the intelligent act.[2] This study is an investigation of the hypothesis that ability in speed is a fundamental factor in the mental act and that the theory of general speed ability is valid.

At the outset we must recognize that even with the simplest mental response itself one is dealing with an extremely complicated process. Great care must be used in dealing with the complexity of materials and types of subjects in attempting to overcome this difficulty. A subject of known intellectual ability may react very slowly in a test because of temperamental peculiarities. On the other hand, the slowness may be due to a certain type of training, going far back

*Recommended by E. G. Boring and received for publication by Carl Murchison of the Editorial Board, October 2, 1928.

[1]For illustration see (26).

[2]Thorndike (24) suggests that ability be analyzed into level, range, and speed.

into childhood, for example, training to check carefully every response or to be slow but accurate. Lack of finger dexterity may tend to slow down responses involving writing, thus concealing the innate quickness of the subject. Downey (7) asserts that graphic speed is symptomatic of general bodily speed of movement and that it has general characterial significance. But, in spite of this theory, practice, familiarity with the material, and many other kindred things may hide the real nature of the response.

One must keep in mind the fact that speed is not the same as time in the mental reaction. Time is the total length of a reaction with possible distraction, fluctuation of attention, or what-not entering into the lapse of time. Theoretically speed may be the actual length of time required for the response free from distractions or anything outside the actual striving for the answer. Time, as it is usually recorded, then is speed plus disturbing ideas, fluctuation of attention, or any one of many possible factors. Let us assume that two subjects, *A* and *B*, are confronted with a mental task, and consider the possibilities which might take place: (1) If it took *A* 20 seconds to react and *B* 25 seconds, without any distractions in either case, one would have a perfect result for the theory of speed ability, since *B* would be inherently 5 seconds slower than *A*. (2) If it took *A* 25 seconds to react correctly but during 5 seconds of that time his attention fluctuates, then the reaction would not be a measure of ability in respect to speed. (3) Another possibility is that *A* goes along the correct pathway for 15 seconds and goes astray for 10 seconds, finally reaching the solution after 30 seconds. (4) As is often the case, *A* may strive correctly during the first 10 seconds and then go astray. At the end of 20 seconds he stops, ***thinking*** he has finished correctly, when in reality he is incorrect. (5) A final possibility is that *A* may start off on the wrong pathway due to wrong determinants, association paths, or any one of many causes.

It is clear from this brief analysis that, theoretically, one cannot measure the speed factor of a mental response precisely, since there is no way to control or show objectively what is actually taking place during the time consumed by a subject. Practically, however, one can attempt to locate the wasted time (1) by pushing the analysis as far as possible into each item responded to, (2) by controlling distractions and constancy of effort by laboratory technique, or (3) by appealing to gross introspection to attempt to find out what actu-

ally took place. To pin down this factor of wasted time more precisely seems to be a fundamental problem and indeed a difficult one. How should it be approached?

In the first place, accuracy must be kept constant by discarding all the items which are responded to incorrectly. It seems as if a subject should be given credit for his degree of accuracy, but obviously that is impossible. On the other hand, it is difficult to set a time limit for each reaction, for then accuracy could not be kept constant because of individual differences in time of responses. Theoretically, if one knew that subject *A* could react correctly in 20 seconds, and *B* correctly in 25 seconds, then one might force *B* to answer correctly by giving him 120% of the time given to *A*. This would be difficult to carry out in a practical way. The possibility of having a subject press a key during a period of distraction or fluctuation of attention has also been of little value in locating wasted time during a mental response.

In the light of the above discussion, it is obvious that the second consideration involved in locating the wasted time must center in the timing of each individual item. One must push the analysis back to a simple mental reaction to see if there are individual differences in speed. The simpler the act, the less likely it is that irrelevant factors will creep into the reaction. Also an adequate number of items or responses must be timed so as to obtain a reliable average time of reaction in the particular ability in question. After simple tasks are given, one might give tasks of varying degrees of complexity under the same conditions of individual timing to attempt to find individual differences in speed for complex reactions. If these differences in speed are relatively the same for simple and complex responses, then one may assume that the complex reaction is made up of simple ones, or, in other words, that there is a constancy in speed for various kinds of mental responses.

The various types of responses bring us to a consideration of the types of tests to be given. In the intelligence test material one finds two kinds of tests which may be classified in a general way as "speed" tests and "all-or-none" tests. Peak and Boring (20) point out that "the all-or-none test is not a test of human performance that does not involve speed, but is simply a test that does not take account of speed" (p. 72). The "all-or-none" tests are educational tests of acquired information devised by certain psychologists wishing to get

a measure of intelligence independent of rate of response. For example, a subject will either know or not know what a *trilobite* is, and reasoning powers will not assist him much. Since an attempt is being made to study the processes involved in ability in respect to speed in the mental reaction, and not in a previously acquired educational response or habit, the tests must be as free as possible from material of this type.

By way of summary, the problem resolves itself into locating the wasted time and in definitely measuring the individual differences in the rate of response to mental tasks. Two questions are to be primarily dealt with: Is the loss of time by the less intelligent subject inherent in the mental reaction? Or is the loss of time in the slower subject due to extraneous factors outside the mental processes involved? If one finds individual differences in reaction-times in pressing analysis to each item of tests, ranging from the simple to the more complex, we may infer that the locus of the differences in respect to speed is in the mental act itself. If one does not find such a relationship, then one must appeal to gross introspection or attempt to assign the differences in speed to any one of a number of factors, such as (1) fluctuation of attention or lack of concentration; (2) differences in interest; (3) various kinds of physical defects or aptitudes, such as finger dexterity due to practice; (4) creative differences; (5) nervousness or temperamental pecularities; (6) desire to be slow but accurate, or habits of checking answers carefully; and (7) perseveration.

During the course of the investigation an attempt will be made to give at least a partial answer, based upon experimental evidence, to the following questions about which so much speculation has centered in the past: (1) To what extent is speed of reaction constant from person to person for different kinds of mental responses? (2) Does speed of reaction vary with responses of different levels of complexity, and is this difference constant for various subjects? (3) What relation, if any, exists between speed in various kinds of responses and mental ability as tested by the typical kinds of intelligence test material?

Methods of Procedure

The previous investigations on the speed factor in mental ability indicated the decided advantage of individual testing over group

testing. The researches of Peak and Boring (20) and Gilbert (11) also illustrated the feasibility of studying rate of response in relation to mental ability, as tested by the mental tests, in the laboratory with a few selected subjects under controlled conditions. Although a great deal of time and patience was required on both sides of the experimental table, the procedure gave assurance of more trustworthy data than that available by other methods. On account of the small number of subjects that could be used in the major part of such a study, it was deemed advisable to check the results by resorting to a larger group, using, however, the same laboratory procedure of timing each item. The incorrect responses were not treated in the results, i.e., when one subject was inaccurate in a particular reaction, that item was thrown out for all the subjects. This procedure was in accordance with the statement on p. 69 that accuracy was to be kept constant.

The data of this investigation are divided into 3 main divisions, based upon the experimental work with 3 different groups of subjects. The data for Study 1 were obtained by giving 4 laboratory subjects 11 different kinds of test material, varying in degree of complexity from the simple auditory reaction to complex "insight" problems, with an average number of 40 items for each test. The data for Study 2, regarded as the most important part of the investigation, were obtained by a far more extensive as well as intensive procedure than that for the first study. Fifteen different kinds of test materials were given to 5 subjects, varying in complexity and number of items from 300 simple auditory reactions to 100 "insight" problems. The large number of items gave assurance of a reliable measure of ability in each test and also allowed a chance for possible practice effects to be eliminated. In Study 3 the results of the two previous investigations were checked by giving 34 subjects individually 10 different kinds of tests varying in degree of complexity, with approximately 30 items in each test. All of the tests were given in the Harvard Psychological Laboratory extending over a period of 3 semesters, from February, 1926 to June, 1927.

In giving every test, the experimenter sat behind a framework of black cardboard, 5 feet long and 4 feet high, which was fastened to the top of a table. The subjects faced this framework and the test material was presented on cards exposed on a sliding frame. The response of the subjects varied in accordance with the test, the answer

being given either vocally or in writing on the exposed card, after the "done" signal had been given. The times were recorded either by the Dunlap chronoscope or a split-second stop-watch. Where the stimulus was presented vocally, the time was recorded in fifths of a second from the final vowel of the stimulus to the first vowel of the response. Where the stimulus was presented visually on the sliding frame, the time was taken from the sound of the sliding frame striking a block (indicating total exposure to the subject) to the "done" signal from the subject. Wherever it was possible the writing of the answers was not included in the time for each item due to possible differences in finger dexterity. An attempt was made to keep constant such factors as number of items presented at each sitting, rest periods, time of day, physical condition of subject, and presentation of stimuli.

Careful instructions peculiar to the individual tests were given with the common instruction, "You are to respond as quickly and as accurately as possible." By this instruction the impression was given each subject that both quickness and accuracy were important. From 3 to 10 trial stimuli were presented to each subject, varying in number with the complexity of the test.

Gross introspective reports revealed a number of significant facts. One subject would unwittingly go back to check an answer because of a previously formed habit. Another responded slowly purposely with the desire to be accurate. Also temperamental peculiarities were revealed which influenced the quickness of response. There were several cases in which the subjects reported fatigue on account of lack of sleep or some physical ailment, such as a headache or poor vision. Where such factors were present the tests were either given over at a later date to all subjects, after an interval sufficient not to involve transfer; or, if the test was of such a nature that repetition was inadvisable, an entirely new test was devised for all the subjects.

With only one or two minor exceptions the attitude of all the subjects may be characterized as excellent. The method of presentation of the stimuli tended to keep the factor of attention constant. The instructions and the obvious desire of the subjects to do well in the tests tended to keep conative factors constant.

Description of Test Material

In the selection of the test materials the following factors were taken into consideration: (1) It was desirable to select the kind of material which could be presented visually or vocally so as to time each item and to avoid as far as possible complicating physical factors, such as finger dexterity in recording the answers. (2) An attempt was made to avoid tests of the educational or "all-or-none" type because of their inadaptability to a study of speed ability. (3) The items in each kind of test material had to be of such a degree of complexity that the task involved a lapse of time long enough to be measured, and yet not so complex as to prohibit giving a large enough number of items to obtain a reliable measure of the particular ability in question. (4) The scope of the tests had to be wide enough to bring various mental functions into play from the simplest to the more complex. (5) The tests were chosen so that a mental reaction free from habitual or acquired factors would be made in each case, i.e., a reaction based upon cognitive striving rather than upon a habitual physiological response. The tests of the more complex type, especially the formboard, maze, sentence completion, and "insight" tests were selected so that the factors such as imagination, originality, or ingenuity would be brought into play. (6) Standardized material was resorted to wherever possible. Every opportunity to profit by the mistakes and findings of previous researches was taken into consideration.

Fifteen different kinds of test materials were used in the three studies. The list given below is based upon the materials presented in Study 2, giving the number of items for each test and the order in which they were presented to the subjects.

Number of test	Number of items	Name of test
1	100	Insight
2	300	Simple Auditory Reaction
3	100	Free Association (Kent-Rosanoff)
4	100	Simple Opposites
5	182	Hard Opposites
6	112	Analogies
7	120	Synonyms
8	62	Simple Mathematical
9	130	Sentence Completion
10	112	Paper Form Board
11	20	Pencil Maze
12	44	Memory
13	40	Discrimination
14	6	O'Connor's Block Test
15	18	O'Connor's Number Sense

For the *Insight Test* 100 "insight" or ingenuity problems were exposed on cards on the sliding frame. Examples of the type of problems may be found in Thurstone's and Givin's Ingenuity Tests (25). This was an excellent test of originality or ingenuity which involved "insight" into certain new relationships on the basis of past experiences. Mathematics entered into the solutions in only a few cases and then it was of the simplest sort.

In the *Simple Auditory Reaction Test* the Dunlap chronoscope was used to record the reactions.[3] After the "Ready" signal was given the interval between the stimuli was varied in length according to a random order to prevent the observer from reacting after a certain interval to which he had become accustomed because of its regularity of occurrence.[4]

The 100 Kent-Rosanoff[5] stimulus words were used in the *Free Association Test.* The procedure was the same as in the *Simple Auditory Reaction Test* discussed above, since the Dunlap chronoscope was used to time the responses. One hundred stimulus words were taken from Burt (5) for the *Simple Opposites Test.* The Dunlap chronoscope was used to record the length of the responses. One hundred and eighty-two stimulus words were presented for the *Hard Opposites Test,* 132 being taken from Wylie (30) and 50 from Pyle,[6] the time being recorded in fifths of a second. The *Analogies Test* was composed of 100 sentences taken from Burt (5). One hundred and twenty items were presented in the *Synonyms Test;* 50 were taken from Burt (5), 30 from DeCamp, and 40 from the Ohio State University Psychological Test.

The *Simple Mathematical Test* consisted of 62 series of 4 simple mathematics problems; 1 addition, 1 subtraction, 1 multiplication, and 1 division, the series being similar to the ones used by Burt (5).

For the *Sentence Completion Test* 130 sentences of the type found

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[3]For a complete description of this chronoscope see (9, 10).

[4]The clutch circuit connected with the voice keys of the chronoscope, to which was attached the electromagnet throwing the clutch in and out, was operated by 15 volts from a switch board of Edison Nickel-Iron Alkaline Storage batteries (direct current). The motor circuit of the chronoscope was operated by a 60-cycle alternating current furnished by the Cambridge Electric Light Co. This current varies less than ½%. The hand on the dial of this chronoscope makes 12 rotations per second with a 60-cycle alternating current. The measuring unit on the dial, therefore, is 1/1200 of a second, or .83.

[5]For full treatment of the Kent-Rosanoff test see (28, pp. 53-71).

[6]See (28).

in Burt (5) and Whipple (28) were exposed on the sliding frame. There were from 3 to 5 blank spaces in each sentence requiring only one word to be written in each space. One hundred and twelve items were exposed separately in the *Paper Form Board Test* (16). This test, like the *Sentence Completion Test,* was excellent for this study, since each involved an element of ingenuity. The *Memory Test* was one for serial impressions of words, numbers, and letters. Of the 44 series, 12 were figures to be repeated forward and 10 were figures to be repeated backwards taken from Wells (27), 10 were letters forward arranged by the author, and 12 were concrete and abstract words found in Whipple (28, p. 86). In the *Pencil Maze Test* the subject threaded the maze on the exposed card by drawing a pencil mark to indicate the path taken. Of the 20 tests given, 4 were taken from Porteus Maze Test (cf. 5, pp. 242-256) and the rest from Dudeney (8, p. 127). For the *Discrimination Test* 40 cards were exposed on the sliding frame with letters or numbers to be checked with a pencil. Fourteen of the cards were printed numbers taken from Woodworth and Wells (29); 8, nonsense material; 12, typed English words; and 6, printed French words arranged by the author.

The last two tests were the *O'Connor Number Sense* (17) and the *O'Connor Blocks* (18). The first correlates highly with clerical ability and the latter highly with mechanical ability. Mr. Johnson O'Connor, of the Lynn, Massachusetts, General Electric Plant, has given the latter test to over 4000 subjects, so his findings are based on reliable experimentation. The *Number Sense Test* is composed of two columns of figures, some being the same and others different. The subject was instructed to check as quickly and as accurately as possible in the "same" column where the numbers were similar and in the "different" column where the numbers were different. The *O'Connor Blocks* consist of 9 pieces of wood cut in a wave-like fashion which can be fitted together to form a solid block. Each subject was given 6 attempts. These two tests were not satisfactory for an investigation of general speed ability, because they test highly specialized aptitudes, and consequently will not correlate with the ordinary mental test material.

Results and Interpretation of Data

Study 1

In the preliminary study 11 different tests, with an average of 40

items for each test, were given to the 4 laboratory subjects, A, B, C, and D.[7]

One of the purposes of this investigation was to study the interrelationship of ability in respect to speed in different mental tasks varying in degree of complexity from the simple auditory reaction to insight problems. In order to have an objective basis for arranging the tests as simple or complex, we have assumed, along with previous investigators of the speed problem (21, pp. 28-29) that the length of time required for a mental response is proportional to its complexity. The tests were classified according to this criterion by totaling the average or mean scores for all the subjects in each test and then dividing by the number of subjects. This gave a mean score in seconds for every item of each test as follows:

Reacting to auditory stimuli	0.333
Giving free association words	1.61
Responding with opposite words	2.65
Giving words of analogous meaning	4.41
Giving words of synonymous meaning	4.64
Completing sentences	11.4
Drawing formboard figures	20.1
Repeating memory test stimuli	26.3
Working simple mathematical problems	35.2
Threading mazes	42.4
Solving insight problems	82.0

All of the items were presented and timed separately for all subjects, and, in order to keep accuracy constant, the incorrect items were discarded.[8] The number of incorrect responses made by each observer is indicated in the table below. The claims of the slow but accurate subject do not seem to be substantiated by the evidence presented. As will be shown later, the speed ranking of the subjects are A first, B second, C third, and D fourth. Subject D, the lowest, made 4 times as many incorrect responses as A, the fastest subject; Subject C, twice as many mistakes as A; and B, 10 more mistakes than A. The implication is that observer A is not only the fastest but is also the most accurate, and the same relationship exists between the remaining 3 subjects, B, C, and D, as shown in the following table:

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[7]These letters were assigned to the subjects after the computation of the data so that the order A, B, C, D, might be the average order of ability in speed.

[8]See page 69.

Tests	A	B	C	D
Opposites	2	2	3	4
Analogies	0	1	2	2
Synonyms	0	0	2	3
Sentence Completion	2	2	3	6
Form Board	0	1	1	2
Memory	9	12	20	34
Mathematical	4	6	1	7
Maze	0	0	0	1
Insight	1	4	4	10
Total number of mistakes	18	28	36	69

After the elimination of the incorrect responses the reaction times for the 11 tests were totalled for each subject. Table 1 shows the means and the probable errors of the means of the 11 tests for the 4 subjects, A, B, C, and D, calculated from the times of the correct items in seconds.

TABLE 1

MEANS AND PROBABLE ERRORS OF THE MEANS FOR THE 11 TESTS

No. of items	Tests	A		B		C		D	
100	Simple Reaction	0.294	0.13	0.312	0.28	0.338	0.40	0.388	0.31
50	Free Association	1.45	0.31	1.62	0.25	1.22	0.19	1.57	0.36
50	Opposites	1.87	0.47	2.24	0.49	2.69	0.88	3.48	1.16
25	Analogies	3.40	0.74	3.72	1.05	5.10	1.93	5.46	2.09
25	Synonyms	3.32	1.67	3.60	1.66	5.36	2.73	6.28	3.80
35	Sentence Completion	7.5	0.33	7.7	0.49	12.9	0.84	17.5	1.43
30	Form Board	13.9	1.00	15.8	1.40	24.6	1.60	26.0	2.10
38	Memory*	35.2		38.5		41.0		54.8	
20	Mathematical	33.1	0.55	32.6	0.99	46.1	1.04	29.1	0.98
12	Maze	31.2	3.8	38.5	5.0	41.0	4.4	45.8	5.8
35	Insight	101.5	9.04	135.7	11.4	144.0	11.2	175.1	14.8

*In the Memory Test the total score was divided by 2 times the number of errors and that figure was then divided by 38, the number of memory series. The probable error and the significance of these differences were not calculated because there was no satisfactory method of treating the different series as individual items.

An inspection of Table 1 reveals the fact that with only two exceptions, the Free Association and Simple Mathematical Tests, the relative rankings of the 4 subjects based upon the means are A first, B second, C third, and D fourth. The Free Association test is a poor one to use in studying ability in respect to speed because of the ab-

normal length of responses to words connected with emotional "complexes" for certain subjects. One might also question the inclusion of the Simple Mathematical Test because of the fact that Subject D reported a great deal of recent practice with simple mathematics, and Subjects A, B, and C reported that they had done very little figuring of a nature that might affect this test. The results in Table 1 seem to indicate that there is a pronounced constancy in the rankings of the subjects and that the relative rate of response for these 4 subjects in mental tasks, varying in degree of complexity from the simple reaction to insight problems, is maintained with marked regularity. In order to show this more clearly, the rankings of the subjects according to the means are brought together in the following table, using the figures 1, 2, 3, and 4, to indicate the relative ranking:

Tests	A	B	C	D
Simple Reaction	1	2	3	4
Free Association	2	3	1	4
Opposites	1	2	3	4
Analogies	1	2	3	4
Synonyms	1	2	3	4
Sentence Completion	1	2	3	4
Form Board	1	2	3	4
Memory	1	2	3	4
Mathematical	3	2	4	1
Maze	1	2	3	4
Insight	1	2	3	4
Total	14	23	32	41

The Spearman Formula, $1 - \frac{6 \Sigma D^2}{N(N^2-1)}$, was used to obtain the rank correlations for each of the 11 tests with all of the others based upon the means. The correlations were all 1.00 with the exception of the Simple Mathematical and Free Association Tests, which were .40 and —.40, respectively.

In order to check the above results one may resort to another method of finding the relative speed rankings for these 4 subjects. In Table 1 the ranking is based upon the means of the time required to react to every item. It is conceivable that this may give a false impression of the ability in speed. Subject B, for example, may be faster than A in 45 out of a possible 50 items in a test, but in those 5 items B may be so much slower that the average or mean time would be unduly affected. In such a case A would be ranked ahead of B

when in reality B was faster in 45 out of a possible 50 items. In order to avoid a possible misrepresentation of this kind, the subjects were ranked on the basis of speed for each item of every test; for example, in Item 1 of the Maze Test, A was second; B, first; D, third; and C, fourth. The totals of the ranks for the individual items of each test have been brought together in Table 2.

TABLE 2

SPEED RANKINGS BASED ON THE TOTALS OF THE RANKS FOR THE INDIVIDUAL ITEMS OF EACH TEST

Tests*	A	B	C	D
Simple Reaction	156	202	262	376
Free Association	108	146	93	154
Opposites	89	118	146	147
Synonyms	46	55	71	78
Analogies	44	50	72	84
Sentence Completion	66	61	106	107
Form Board	51	58	90	101
Maze	22	26	30	42
Mathematical	48	47	75	30
Insight	65	86	90	109

*The Memory Test is not included because it was impossible to rank the items according to speed in each series or memory span.

An inspection of Table 2 shows that in the Sentence Completion Test Subject B was faster than A when the totals of the ranks for the individual items were calculated. In all of the other tests, however, the subjects maintain their rankings as shown in Table 1. In the Sentence Completion Test, therefore, B completed a larger number of sentences in shorter time than A, but B's average reaction-time was longer because of the extra time required to complete a number of the more difficult sentences. With this one exception, the relative constancy of the speed ranking is as marked as in Table 1.

In order to present these ranks for the individual items in a statistical way, the rankings of the subjects have been tabulated below as if each item of all the tests were a separate test. The following figures include all of the items, a total of 382, exclusive of the Memory Test:

Rank	A	B	C	D
First	184	114	56	21
Second	118	128	87	65
Third	53	93	146	92
Fourth	27	47	93	204

It seems evident from the above table that the same tendency to constancy in relative speed exists when the items are treated as separate tests. Subject A, for example, is first 184 times and fourth only 27 times out of a possible 382, while at the other extreme D is fourth 204 times and first only 21 times out of a possible 382. Although the subjects are not so widely separated in speed that the first, A, is never last for any item, nor the last, D, is ever first, the differences are very marked, indicating a high degree of constancy in speed.

The next step in the treatment of the data is to resort to the usual statistical procedure for the demonstration of the significance of the differences between the means of the speed rankings of the subjects. This procedure is necessary since the reliability of the ranking obviously depends on whether the differences are due to chance or whether they are statistically reliable.[9] In Table 5 are to be found the ratios, for each pair of subjects in each test (exclusive of the Memory Test), between the difference and the probable error of that difference.

It is not possible to say dogmatically whether these differences are significant, since significance is relative and not absolute. If one takes as low a value for $\frac{Diff.}{P.E._{diff.}}$ as 2.5 to represent a probably significant difference, 42 of the total of 60 differences for the 10 tests are significant and 30 of the differences which are significant are as great as 40 or above (2, 3, 4, 14). Where the difference between any two subjects is not significant they are presumably on a par with each other and to rank one above the other as has been done here may falsely interpret the data. Thus, where the differences are not significant, it would seem that the subjects should be ranked the same. This is impossible, for in the sentence completion test, as an illustration, the difference between A and B is not significant, but the differences between A and C and A and D are significant; and also these differences are significantly greater than the differences between B and C and B and D. If we take the final ranking A, B, C, D of the 18 differences which are not significant, only one inversion[10] can be found which does not support the ranking given in Table 1. Taken as a whole, the conclusion may be drawn from the data that the differences are significant and that there is justification for saying that the statistical analysis bears out the relative constancy of the ability in speed for these 4 subjects.

[9]For a treatment of this method, see (19).
[10]Cf. Form Board Test for significance of differences, Table 3.

TABLE 3

SIGNIFICANCE OF DIFFERENCES

The figures represent ratios ($\frac{Diff.}{P.E._{diff.}}$) of the difference to its probable error for the average times for every pair of subjects

	Simple Reaction				Free Association		
	B	C	D		B	C	D
A	5.75	10.40	24.83	A	1.53	3.81	2.59
B		5.53	27.84	B		6.34	1.43
C			9.66	C			6.41
	Opposites				Analogies		
	B	C	D		B	C	D
A	2.70	4.22	6.45	A	1.63	4.38	4.50
B		2.04	4.13	B		3.19	3.64
C			2.72	C			.06
	Synonyms				Sentence Completion		
	B	C	D		B	C	D
A	.086	3.52	7.31	A	.04	6.00	7.03
B		2.77	11.47	B		5.36	6.62
C			1.81	C			7.69
	Form Board				Mathematics		
	B	C	D		B	C	D
A	1.17	5.85	5.63	A	.08	11.01	3.57
B		4.24	4.36	B		9.79	2.90
C			.08	C			11.97
	Maze				Insight		
	B	C	D		B	C	D
A	1.16	1.72	3.48	A	2.36	3.14	4.28
B		.05	3.01	B		.03	2.13
C			2.02	C			2.00

Study 2

After finding such a marked constancy for ability in respect to speed in the mental tasks outlined in Study 1, it seemed desirable to check the results by repeating the experiment with a similar laboratory group, using a wider range of tests with a larger number of items in each test. In this major part of the investigation there were 5 laboratory subjects, A, B, C, D, and E[11], graduate students of 2 years' experience or longer in laboratory work. The average number of correct items for the first 13 tests was approximately 90. There were 30 sittings of 1 hour each for each subject at a week's interval.

So as to classify and number the tests according to the complexity, the same method was used here as in Study 1, i.e., complexity is based upon the relative time required to react to the various kinds of tests.[12] The average or mean score for all the subjects in each

[11]See footnote 7.

[12]Cf. Study 1, page 75.

test was totalled and then divided by the number of subjects. This gave a mean score for every item for each test as follows:

Simple Auditory Reaction	0.290 seconds
Free Association Words	1.72 "
Simple Opposites	2.61 "
Hard Opposites	4.37 "
Analogous Words	5.05 "
Synonymous Words	7.21 "
Paper Form Board	20.7 "
Sentence Completion	20.9 "
Memory Series	31.4 "
Simple Mathematical	43.1 "
Maze Threading	44.3 "
Discrimination	153.0 "
Insight Problems	181.0 "
Number Sense	57.2 "
O'Connor Blocks	186.0 "

Before proceeding with the interpretation of the data, it is interesting to consider the claims of the slow but accurate subjects, as was done in Study 1, page 77. The number of mistakes made by each subject in the tests where items had to be discarded because of inaccuracies are shown as follows:

Tests	A	B	C	D	E
Hard Opposites	9	8	10	15	13
Synonyms	5	8	12	14	14
Form Board	20	6	10	7	13
Simple Mathematical	11	12	16	4	19
Memory	9	30	32	25	20
Discrimination	11	18	24	19	30
Insight	2	4	7	8	10
Number Sense	3	5	7	0	0
Total number of mistakes	70	91	118	92	119

As will be shown later, the rank order according to speed is A, B, C, D, and E. Subject A, the fastest, made a total of 70 mistakes in the 8 tests, while E, the slowest, made 119. With the exception of D, the fastest subjects seem to hold a relative position in regard to accuracy, the same as in Study 1, page 77. D's ranking here is largely due to the Simple Mathematical Test where he was the fastest and far more accurate than the other subjects.[13] In the Num-

[13]It was discovered that D's and E's superiority in this test was due to a great amount of practice with figures at the time the test was given.

ber Sense Test of clerical ability, where D and E excelled, they made no mistakes. Although the results are not so clear-cut as in Study 1, the data seem to indicate that the fastest subjects in a particular ability tend to be the most accurate.

Afer the elimination of the items which were responded to incorrectly, the times required to make the reactions in the 15 different tests were totalled for each observer. From those data the means for the 5 subjects, A, B, C, D, and E, were calculated for the 15 tests.

TABLE 4

SPEED RANKING BASED UPON MEANS—TIMES IN SECONDS

No. of items	Tests	A	B	C	D	E
300	Simple Reaction	0.197	0.267	0.272	0.302	0.412
100	Free Association	1.14	1.27	1.38	1.29	1.74
100	Simple Opposites	2.88	3.16	3.32	4.92	6.60
150	Hard Opposites	4.76	5.52	6.12	6.40	8.12
100	Analogies	6.76	7.40	7.84	8.40	9.96
90	Synonyms	6.76	7.52	8.40	10.64	9.16
88	Form Board	11.6	15.5	15.2	17.1	23.2
60	Sentence Completion	11.9	14.3	16.2	18.3	20.6
38	Memory	19.8	30.4	23.9	24.6	26.9
62	Mathematical	33.	38.4	52.4	29.5	26.9
19	Maze	18.4	33.5	36.	36.9	51.8
40	Discrimination	91.2	97.5	103.4	125.	133.3
90	Insight	48.7	88.8	116.	114.	147.
	O'Connor Number Sense	107.	77.	79.	102.	114.
	O'Connor Blocks	111.3	114.8	84.5	93.5	288.1

In order to show more clearly the rankings of the 5 subjects based upon the means, the data is brought together below, using the figures 1, 2, 3, 4, and 5 to indicate the ranks rather than the means themselves.

Upon inspection of the above ranking, one finds that there are 8 tests where there are inversions of rank order from a perfect rank order of A first, B second, C third, D fourth, and E fifth. Three of these inversions are of a serious nature, the Simple Mathematical, the Number Sense, and the O'Connor Blocks. The other 5 are only one-place inversions between subjects who rank next to each other, the Free Association, Synonyms, Form Board, Memory, and Insight. With the above exceptions, the relative speed rankings of the 5 subjects are maintained with marked regularity the same as in Study 1 (page 77).

Tests	A	B	C	D	E
Simple Reaction	1	2	3	4	5
Free Association*	1	2	4	3	5
Simple Opposites	1	2	3	4	5
Hard Opposites	1	2	3	4	5
Analogies	1	2	3	4	5
Synonyms*	1	2	3	5	4
Paper Form Board*	1	3	2	4	5
Sentence Completion	1	2	3	4	5
Memory*	1	5	2	3	4
Pencil Maze	1	2	3	4	5
Simple Mathematical*	3	4	5	2	1
Discrimination	1	2	3	4	5
Insight*	1	2	4	3	5
Number Sense*	4	1	2	3	5
Blocks*	3	4	1	2	5
Total	22	37	44	53	69

* Involves an inversion.

The above results may be checked by resorting to another method in finding the relative speed rankings. As was found in Study 1 (page 77), the speed ranking based upon the means may give a false interpretation of the data because one subject may be faster than another in 90 out of a possible 100 cases, but in those 10 items the first subject may be so much slower as to affect his average unduly. In order to find out if such misrepresentation had taken place, the subjects were ranked on the basis of ability in speed for each item of every test. The totals of the ranks for the individual items of each test have been brought together in Table 5.

An inspection of Table 5 shows in the case of the "insight" problems that C was faster than D when the totals of the ranks for the individual items were calculated. This changed the rank order of the 5 subjects so as to favor the normal relative ranking as a whole. In 5 other tests one finds slight inversions of one place only for the Free Associations, Hard Opposites, Analogies, Sentence Completions, and Maze tests. In these 5 tests the speed rankings are therefore not as constant relatively as when based upon the means shown in Table 4. In the other 8 tests the same relative speed ranking is maintained as shown above.

But since inspection of this sort is sometimes dangerous, let us resort to the statistical method in ranking the subjects as already tabulated in Table 5, i.e., upon the speed rankings of the individual items. If one treats every item of all the tests, exclusive of the 300

TABLE 5

SPEED RANKINGS BASED ON THE TOTALS FOR THE INDIVIDUAL ITEMS OF EACH TEST

Tests*	A	B	C	D	E
Free Association†	233	282	301	279	401
Simple Opposites	161	252	257	396	445
Hard Opposites†	316	425	422	488	579
Analogies†	210	290	281	354	373
Synonyms	223	258	264	307	285
Form Board	152	262	246	262	369
Sentence Completion†	117	171	194	166	251
Maze†	21	52	63	62	87
Mathematical	174	244	297	131	94
Discrimination	73	80	100	167	180
Insight†	144	236	288	330	381
Number Sense	13	4	5	11	12
Blocks	18	19	8	15	30

*The Simple Reaction and the Memory Test are omitted because of the similarity of the stimuli in the former and the impossibility of timing each item of a memory span.

†Involves an inversion.

Simple Reactions, a total of 859, as if they were separate tests, one finds the relative speed rankings of the subjects to be as follows:

Rank	A	B	C	D	E
First	411	115	119	127	87
Second	169	242	202	153	91
Third	140	246	228	144	111
Fourth	89	171	156	240	196
Fifth	50	85	154	195	374

It is obvious from the above table that the same tendency to constancy in relative speed exists when the items are treated as separate tests. Subject A, for example, is first 411 times and fifth only 50 times out of a possible 859, while D is fifth 374 times and first only 87 times. The 3 middle subjects maintain their relative positions with marked regularity, i.e., B has the largest number of seconds, D the largest number of fourths, and C, although not having the largest number of thirds, takes a middle position between the other 4 subjects. This check upon the results in Table 4 strengthens

the position that there is a high degree of constancy in ability in respect to speed for these 5 subjects.

The intercorrelations of each of the 15 tests with all of the others were calculated by the Spearman formula, 115 out of a possible 210 correlations were .90 or above. All of the correlations were above .70 with the exception of the Simple Mathematical Test, which correlated negatively, the Number Sense Test, and the O'Connor Block Test, which correlated between .30 and .40.

There seems to be justifiable basis for discounting the negative correlations of the Simple Mathematical Test, since two subjects reported a great amount of practice in figures, and since Subject C stated that he habitually checked his figures before giving the "done" signal, which undoubtedly lengthened his responses. The subjects who work with rote memory, obviously D and E in this test, are certain to be much quicker than the subjects who have to stop to think about a certain multiplication or division. One would also be inclined to attach little significance to the low correlations for the Number Sense and the O'Connor Blocks Tests, since they measure highly specialized aptitudes, the former clerical ability and the latter mechanical ability.[14]

A good illustration of how practice or familiarity with the test material may tend to conceal the speed factor can be found in the Sentence Completion Test. The subjects were given 70 sentences to complete taken from H. T. Moore (1). The ranking of the subjects based upon the means was A, 12.2 seconds; B, 15.1; C, 18.7; D, 14.5; and E, 20.4. D reported an intimate familiarity with the material so these items were discarded and a new test devised. In the presentation of the unfamiliar test, D, who ranked second in the first series of sentence completions, ranked fourth, the normal position he has held throughout the series of tests as shown in Table 4. These illustrations tend to uphold the experience of previous investigators in that the more careful one is in isolating speed from factors which seem to conceal it, the greater the constancy one finds in the relative speed ranking of the subject.

The last step in the treatment of the data in this study is to resort to the usual statistical procedure to find out if the differences between the means of the speed rankings of the subjects as shown in Table 4 are significant. The reliability of the rankings will be ascertained

[14]Cf. page 75.

TABLE 6

SIGNIFICANCE OF DIFFERENCES IN STUDY 2

The figures represent ratios $\left(\frac{Diff.}{P.E._{diff.}}\right)$ of the difference to its probable error for the average times for every pair of subjects

	Number Sense					Free Association					Simple Opposites			
	B	C	D	E		B	C	D	E		B	C	D	E
A	5.64	6.13	.90	.70	A	5.26	8.33	4.40	16.96	A	5.22	4.44	15.93	10.58
B		.83	5.55	4.25	B		5.24	.88	15.16	B		3.12	15.0	9.86
C			5.91	4.23	C			3.88	10.59	C			18.10	7.07
D				1.89	D				12.50	D				4.56
	Hard Opposites					Analogies					Synonyms			
	B	C	D	E		B	C	D	E		B	C	D	E
A	5.18	5.68	5.70	11.50	A	4.78	5.56	7.19	8.60	A	1.78	2.81	5.57	4.27
B		3.51	4.00	7.36	B		3.03	4.85	6.90	B		1.78	5.01	3.50
C			1.20	5.68	C			3.01	5.30	C			3.24	1.30
D				4.50	D				3.90	D				2.27
	Form Board					Sentence Completion					Simple Mathematical			
	B	C	D	E		B	C	D	E		B	C	D	E
A	4.68	4.60	4.70	8.92	A	2.09	4.24	4.31	6.88	A	6.41	16.13	4.22	9.00
B		2.03	1.63	5.98	B		1.64	2.52	4.66	B		11.3	9.07	16.3
C			1.42	5.78	C			1.40	3.61	C			17.99	22.8
D				4.22	D				1.56	D				3.14
	Maze					Discrimination					Insight			
	B	C	D	E		B	C	D	E		B	C	D	E
A	3.92	3.40	3.96	6.57	A	2.01	3.21	7.06	8.23	A	7.14	9.28	7.28	17.0
B		.006	.070	3.00	B		1.46	6.37	9.09	B		3.58	2.69	9.35
C			.00	2.70	C			4.44	5.38	C			1.94	4.00
D				2.60	D				1.69	D				3.52

if one finds that the speed ranking is not due to chance. The significance of the differences here should be much more reliable than those of Study 1 in that the number of items are larger in each test. In Table 6 are to be found the ratios for each pair of subjects in each test between the difference and the probable error of that difference.[15]

As stated above,[16] it is impossible to say dogmatically whether these differences are significant, since significance is relative and not absolute. If one takes a value of 3 for $\frac{Diff.}{P.E._{diff.}}$ one finds 92 out of a possible 120 differences are significant. An inspection of Table 6 shows that 74 of these differences are above 4, and 55 of them are above 5.

The evidence presented shows quite conclusively that the speed rankings of the 5 subjects as A first, B second, C third, D fourth, and E fifth are not due to chance. This statistical procedure has checked the results as presented in Table 4, and it has upheld the findings of Study 1. From these two studies one may conclude with assurance that for these 9 subjects, 4 in Study 1 and 5 in Study 2, the relative constancy of ability in respect to speed is very marked in mental tasks of various degrees of complexity. Furthermore, the individual differences in these speed rankings between subjects are based upon differences which are statistically significant.

Study 3

After finding such pronounced constancy in the speed relationships in Studies 1 and 2 for the two laboratory groups, it seemed desirable to check the results by carrying on a similar experiment with a larger group, using the same amount of care by giving the tests individually under the same laboratory conditions and by carefully timing the individual items of each test. Thirty-four Harvard students of advanced or graduate experience in laboratory technique and methods were used as subjects. Each subject gave 2 hours to the 10 different tests of the experiment, an hour at a sitting. The approximate number of items in each test was 30, with the exception of the

[15]The Simple Reaction test is omitted due to the similarity of the stimuli, the Memory Test because of the impossibility of keeping accuracy constant for each series, and the O'Connor Blocks because of the small number of trials (6) upon which to base the mean and probable error of the mean.

[16]Cf. page 80.

Simple Reaction Test, which was based upon 100 auditory reactions with the Dunlap chronoscope. The 10 tests, listed in the order of presentation, were as follows: Simple Auditory Reaction, Form Board, Maze, Insight, Sentence Completion, Opposites, Synonyms, Discrimination, O'Connor Number Sense, and Memory Test.

The number of items in each test which had to be discarded because of inaccuracies was not very great. Although the quality of the answers varied in the Opposites, Synonyms, and Sentence Completion tests, all subjects made accurate responses. The inaccuracies in the Discrimination, Number Sense, and Memory tests were taken account of in scoring. For the Form Board, Maze and Insight tests 5, 3, and 4 items, respectively, were discarded because of inaccuracies. If one divides the group of 34 subjects in half, based upon quickness for the later 6 tests mentioned above, the second 17 subjects made 4 times as many mistakes as the first 17. If one divides the group into quarters, the last quarter was 6 times as inaccurate as the first, and the third 3 times as inaccurate. The majority of the errors fell in the scores of the slowest 10 subjects. The results in general bear out the findings in Studies 1 and 2, that the quickest subjects are also the most accurate.

After the inaccurate responses had been discarded, the times for the individual items in each test for the 34 subjects were totalled and from these scores the correlations of each test with all the others was obtained by the Pearson product moment formula (19).

In Table 8 the order of the tests has been arranged so as to form a hierarchy, taking the insight test as a basis for the other orders. This arrangement indicates the probable existence of a common factor as proposed by Spearman (12, 22, 23). The table should be read from left to right along each column starting at the top. Out of a possible 80 there are 20 inversions and 8 of the inversions are not large, being only 4 points or below. It should be noted that the inversions are most frequent in the Number Sense Test which measures a highly specialized aptitude.

In the light of the above hierarchical arrangement of the correlations one may draw the following conclusions. Spearman maintains that all speed measures involve general ability or *g* (12, 22, 23). He also states that correlations of general ability conform to the hierarchical arrangement (23, p. 74). Granting Spearman these assertions, then it follows that speed measures must correlate according to a hierarchy. The correlations of speed obtained in this re-

TABLE 7

INTERCORRELATIONS AMONG THE 10 TESTS FOR THE 34 SUBJECTS IN STUDY 3
P.E.'s given below each *r*

	Simple Reaction	Form Board	Maze	Insight	Sentence Completion	Opposites	Synonyms	Discrimination	Number Sense	Memory
Reaction		.637	.617	.766	.713	.632	.580	.376	.113	.593
		±.066	±.073	±.049	±.058	.066	.073	.086	.113	.078
Form Board	.637		.849	.869	.722	.618	.497	.386	—.002	.672
	.066		.036	.036	.058	.073	.086	.096	.114	.058
Maze	.617	.849		.880	.767	.594	.481	.505	.239	.711
	.073	.036		.021	.046	.073	.086	.086	.110	.076
Insight	.766	.869	.880		.829	.715	.619	.482	.255	.770
	.049	.036	.021		.041	.056	.073	.086	.104	.049
Sent. Comp.	.713	.722	.767	.829		.648	.560	.476	.246	.657
	.058	.058	.046	.041		.066	.073	.086	.110	.066
Opposites	.632	.618	.594	.715	.648		.757	.536	.314	.453
	.066	.073	.073	.056	.066		.049	.086	.104	.086
Synonyms	.580	.497	.481	.619	.560	.757		.468	.234	.499
	.073	.086	.086	.073	.073	.049		.086	.110	.086
Discrimination	.376	.386	.505	.482	.476	.536	.468		.437	.406
	.086	.096	.086	.086	.086	.086	.086		.096	.096
Number Sense	.113	—.002	.239	.255	.246	.314	.234	.437		.104
	.113	.114	.110	.104	.110	.104	.110	.096		.113
Memory	.593	.672	.711	.770	.657	.453	.499	.406	.104	
	.078	.058	.076	.049	.066	.086	.086	.096	.113	

TABLE 8

ORDER OF TESTS ARRANGED SO AS TO FORM A HIERARCHY

	Insight	Maze	Form Board	Sentence Completion	Memory	Simple React.	Opposites	Synonyms	Discrimination	Number Sense
Insight		.88	.86	.82	.77	.76	.71	.61	.48	.25
Maze	.88		.84	.76	.71	.61	.59	.48	.50	.23
Form Board	.86	.84		.72	.67	.63	.61	.49	.38	.00
Sent. Comp.	.82	.76	.72		.65	.71	.64	.56	.47	.24
Memory	.77	.71	.67	.65		.59	.45	.49	.40	.10
Simple React.	.76	.61	.63	.71	.59		.63	.58	.37	.11
Opposites	.71	.59	.61	.64	.45	.63		.75	.53	.31
Synonyms	.61	.48	.49	.56	.49	.58	.75		.46	.23
Discrimination	.48	.50	.38	.47	.40	.37	.53	.46		.43
Number Sense	.25	.23	.0	.24	.10	.11	.31	.23	.43	

search fall into a hierarchy. Therefore the data verify Spearman's conclusion that measures of speed involve general ability.

An examination of the intercorrelations in Table 7 reveals the fact that the correlations are positive and in the large majority of cases positively significant. The lowest correlations occur in the Number Sense Test, the use of which has been seriously questioned above, page 86. The conclusions reached by the author of the test, O'Connor, that it will not correlate with ordinary mental test material and that it measures a highly specialized ability seems to be upheld here. Three of the correlations are below .468 in the Discrimination Test. Five of the subjects who did poorly in this test, relative to their general average, reported great difficulty in checking the numbers in the test because of eye trouble. Also an element of finger dexterity entered into this test more than in any other, which might have tended to conceal the ability in question. These facts seem to indicate that the correlations are even more significant than the table implies at the first glance. If one omits the Number Sense Test, the average correlation is .62, and, if one omits both the Number Sense and Discrimination Tests, the average correlation is .68.

Even if we include the Number Sense Test, 26 out of the possible 45 correlations are above .50, and 20 correlations are above .60. If we exclude the Number Sense Test, which one feels justified in doing, the correlations may be summarized as follows:

	1. to .59	.59 to .40	.40 to .0
Cases	22	12	2
Percentages	61.1	33.3	5.5

The four tests which gave the highest correlations are the Form Board, Maze, Sentence Completion, and Insight Tests. As was stated before, these tests involve originality, and there seemed to be less opportunity for previous experience and acquired habits to enter into the solutions. This fact may be regarded as significant in that these tests are no doubt better measures of mental ability as tested by the ordinary mental test material than the other six tests.

The conclusion to be drawn from the results of Study 3 is that the constancy of speed ability as indicated in the two previous studies is maintained with a reliable degree of accuracy with a group of 34 subjects. These results are all the more significant when one con-

siders the selected nature of the group. Subjects chosen at random, giving a more typical cross section of society in regard to mental ability, would no doubt have revealed even greater individual differences in respect to ability in speed.

Note: Professor E. G. Boring of Harvard University has been interested in applying Spearman's method of the tetrad difference to these data, and he has suggested the addition to this paper of the results of his computations. For the data of Table 9 he obtained the probable error of the tetrad difference and the median of the absolute values of the tetrad differences given under (1) below. He followed the procedure given by C. Spearman (23) and used, for the probable error, formula 16A of the Appendix (p. xi). The closeness of the probable error to the median would be held by Spearman to indicate that there is, at least in an approximate sense, a common factor among these data. On the other hand, the fact that the median exceeds the probable error should, on Spearman's theory, indicate that there is some small degree of overlapping among the special abilities which, independently of speed, are involved in the tests. Since no attempt was made to avoid such overlapping, its presence should create no surprise. However, Boring tried to see whether the relationship could be improved by omitting the test for "number sense" and repeating the computation. This test gives, in general, low correlations with the other tests, and Boring found that it participated in the formation of large tetrad differences more often than any other tests. His results, with the test for "number sense" omitted, are given under (2) below, where it appears that the median still slightly exceeds the probable error. Omitting the results obtained with the paper formboard, the test contributing to the second largest number of large tetrad differences, did not alter the relationship greatly. These data are given under (3) below. It is not clear that Spearman's method ought to be applied where the cases are so few, but the presentation of these results will at least forestall the critic who thinks that the method should have been used.

TABLE 9

	Number of subjects=N	Number of tests=n	Number of tetrad differences	P.E. of tetrad difference	Median of tetrad differences
(1) All 10 tests	34	10	630	0.0590	0.0687
(2) Omitting test for "number sense"	34	9	378	0.0543	0.0600
(3) Omitting also paper form-board	34	8	210	0.0552	0.0635

Conclusions

In this study an attempt has been made to show the relationship between rate of response and mental ability as measured by the mental test material. In this way it was hoped that some light could be thrown on the nature of intelligence by a scientific

approach. The research was planned so that a small group of subjects could be given a wide range of tests with a large number of items, individually timed. Ability in respect to speed was isolated by keeping accuracy constant throughout the series of investigations. The results were checked by resorting to a larger group (34 subjects), maintaining, however, the same laboratory procedure and technique in presenting and timing the items of each test. The data as presented in these three studies justify the following conclusions:

1) The four subjects of Study 1 maintain their relative ranking in respect to speed with marked regularity, as shown in Table 1. This same tendency existed in a pronounced way when the ranks were based upon the means, in time, and upon the total ranks, i.e., as if each item were a separate test.[17] The significance of the individual differences in speed ability were only fairly reliable, probably because of the small number of items presented in each test.

2) In checking the above results in Study 2 in a far more intensive and extensive way, the 5 subjects showed a very marked constancy in relative speed rankings.[18] The significance of the individual differences of the rankings in respect to speed were significant, when treated statistically, for 92 cases out of a possible 120 were equal to or above a $\frac{Diff.}{P.E._{diff.}}$ of 3; 74 of these differences were above 4, and 55 of them were above 5.[19]

3) In Study 3 the results of the two previous studies were confirmed, when 10 different tests were given to 34 subjects. The same methods as in Studies 1 and 2 of individual timing of each item and laboratory technique were used on the larger group. The correlations in regard to quickness of response were significant, 61% being above .50, 33% between .59 and .40, and only 5% below .40.

4) The results seem to show that there is a general factor in the various mental tasks presented to the subjects of this investigation and that the measures of speed obtained involve general ability apropos of Spearman. Granted that Spearman is correct in stating that speed measures involve general ability and that correlations of general ability conform to a hierarchical arrangement, then it follows

[17] See Table 2, also page 79.
[18] See Table 4.
[19] See Table 6.

that the data of this research verify Spearman, for speed measures must correlate according to a hierarchy, and Table 8 shows that such is the case. Therefore the common factor in the present study represents Spearman's general ability.

5) The data and experience obtained from this investigation uphold the conclusions of those investigators who have maintained that the more intensive and controlled a research of ability in respect to speed, the more positively significant are the results. This is especially true of the more recent researches, notably Clarke (6), Hunsicker (13), Peak and Boring (20), and Gilbert (11). These investigations have been conducted, for the most part, by the individual method of timing each item of a test and have conformed to scientific testing procedure. This refined and objective method of studying ability in respect to speed has convinced the experimenter in each case mentioned above of the significant relationship between rate and mental ability.

6) Certain factors, such as finger dexterity, fatigue, and practice effects, tend to hide the actual relative speed ranking of subjects, unless great care is taken in laboratory technique and procedure. These factors can be isolated and controlled only by timing each item of a test given to individuals rather than to groups of subjects.

7) The present study reveals the fact that there are certain tests where ability in respect to speed does not correlate in a reliable way with quickness in other tests because a special aptitude is being measured. This is especially true in the Number Sense Test and the O'Connor Blocks Test,[20] which measure clerical ability and mechanical ability, respectively. It was also noted that in tests like Free Association many of the responses were abnormally long, due to their connection with emotional "complexes." In the use of the Simple Mathematical Test it was discovered that differences in rate of response were due to practice or to previously acquired habits.[21] These factors quite naturally tend to hide the real significance of ability to respond quickly, and thus give rise to poor correlations with rate in the other tests. It is evident that great care should be taken in the selection of the test material.

8) The differences in respect to speed were found to lie in the individual items. As far as could be discovered by experimentation,

[20]See page 75.
[21]See page 83.

the differences in speed inhered in the mental acts themselves, and could not be assigned to gross distractions or irrelevant acts.[22] The data obtained from these tests indicate that the same speed relationship exists between reactions of a varying degree of complexity. The data also indicate that the fastest subjects tend to be most accurate.

9) The results of these three researches, therefore, agree with those of the previous investigators who have held that speed of reaction is one of the most important factors in individual differences in ability to react to mental tasks. It has been shown that subjects will maintain their relative speed rankings in various kinds of mental reactions. Ability in respect to speed, therefore, is an individual trait which is characteristic of mental behavior.

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[22]See page 70.

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ÉTUDE EXPÉRIMENTALE DE LA RELATION ENTRE LA VITESSE ET LA CAPACITÉ MENTALE

(Résumé)

Cette enquête est un essai expérimental de déterminer le rôle de la vitesse dans la capacité mentale. Dans les deux premières études, on a fait subir à quatre et à cinq sujets respectivement quinze types différents de tests variant en complexité et en nombre de 300 réponses auditives simples à 90 problèmes complexes. Dans les deux études les sujets ont maintenu leurs résultats relatifs de vitesse avec une régularité marquée, c'est-à-dire, presque toutes les corrélations ont été au-dessus de, 70. Ces résultats ont été confirmés par une troisième étude où on a fait subir à 34 sujets dix types différents de tests. Les corrélations de capacité par rapport à la vitesse ont été signifiantes, la moyenne étant approximativement de, 68. Dans chaque étude les différences individuelles des résultats par rapport à la capacité de vitesse ont été signifiantes quand traitées d'une façon statistique.

Dans ces études on a présenté chaque partie séparément à chaque sujet individuellement au moyen d'un cadre glissant. On a rendu constante l'exactitude pendant l'enquête au moyen d'écarter les réponses incorrectes. On a

essayé d'éviter les tests éducationnels ou acquis pour avoir une mesure de l'effort mental..

L'analyse des différences de vitesse s'est trouvée dans les parties individuelles des tests. Les données ont bien indiqué qu'avec une technique contrôlée de laboratoire, les différences de vitesse ont été inhérentes aux actes mentaux eux-mêmes, et n'ont pas été dues aux facteurs extérieurs, tels que la distraction, la pratique, la fatigue, ou les particularités de tempérament. Les sujets ont maintenu leurs résultats relatifs de vitesse dans les réponses simples et complexes. Les données ont indiqué l'existence probable d'un facteur général de vitesse comme proposé par Spearman. La capacité de vitesse est donc un trait individuel caractéristique du comportement mental.

McFARLAND

EINE EXPERIMENTELLE STUDIE DER BEZIEHUNG ZWISCHEN SCHNELLIGKEIT UND GEISTIGER FÄHIGKEIT

(Referat)

Diese Untersuchung ist ein experimenteller Versuch die Rolle der Schnelligkeit in geistiger Fähigkeit zu ermitteln. In den ersten beiden Studien wurden vier und fünf, Vpn. je fünfzehn verschiedene Typen von Test Material vorgelegt, das in Bezug auf Schwierigkeit und Anzahl varierte von 300 einfachen mündlichen Antworten bis zu 90 verwickelten geistigen Problemen. In beiden Experimenten hielten die Vpn. ihre verhältnismässige Schnelligkeits-leistungen aufrecht, d. h. die Korrelationen waren fast alle über .70. Diese Resultate wurden in einem dritten Experiment bestätigt, bei dem 34 Vpn. zehn verscheidene Arten von Test Material vorgelegt wurden. Die Korrelationen der Fähigkeit in Bezug auf Schnelligkeit waren bedeutsam. im Durchschnitt fast .68. Bei jedem Experiment waren die Individuellen Unterschiede in den Schnelligkeitsleistungen—statistisch behandelt—von Bedeutung.

In diesen Untersuchungen wurde jede einzelne Aufgabe jeder Vpn. persönlich übergeben und die Schnelligkeit in jedem Fall apart bemessen. Während der ganzen Untersuchung wurde streng auf Genauigkeit gesehen, indem man alle falschen Antworten ausschaltete. Man versuchte alles Schulhafte oder Angelernte zu vermeiden um ein Mass des geistigen Strebens zu erhalten.

Die Analyse bewies dass die Unterschiede in der Schnelligkeit in den persönlichen Items lagen. Die Ergebnisse zeigten sehr deutlich, dass bei der kontrollirten Technik des Laboratoriums die Unterschiede in der Schnelligkeit in den geistigen Vorgängen selber liegen, und nicht durch äussere Faktoren verursacht werden, wie Zerstrenung, Übung, Ermüdung oder persönliche Eigenheiten. Die Vpn. hielten ihre verhältnismässigen Schnelligkeits-leistungen in den einfachen sowie in den schwierigen Antworten bei. Die Ergebnisse deuten auf einen wahrscheinlichen allgemeinen Schnelligkeits-Faktor hin wie Spearman annimmt. Fähigkeit in Bezug auf Schnelligkeit ist also ein persönlicher Charakterzug, der charakteristisch für das geistige Verhalten ist.

McFARLAND

THE INFLUENCE OF ATTITUDE ON LEARNING*

From the Psychological Laboratory of Cornell University

G. L. Freeman

In this study we were interested primarily in the facilitation of our *O*'s in the performance of a particular task, viz., the association of every one of four common musical chords with a particular reaction key. For this purpose we tried two radically different procedures, the latter and more successful one having been suggested by reports taken under the first. These reports indicated the great importance played by attitude in the type of learning required; and the specific problem of this paper, as indicated by its title, is the part played by the attitude of the *O* under our two procedures.

While the experiment we are to describe was not intended to add evidence either for or against the "configural" theory of learning, the questions of attitude and structure are inevitably linked. In a paragraph following the presentation of the results, we shall discuss the relation which we feel to be apparent.

General Technique, Apparatus, and Observers

The experiments were conducted in a semi-sound proof room about 20 by 40 feet in size. Two observer's booths were placed at one end and an auditory apparatus at the other end of the room. The chords were produced by Stern variators Nos. 4, 5, and 6. These chords were actuated by the air of a compressed system kept at a constant pressure, and were tuned by comparison with standard König forks at a room temperature of 71 degrees. No interference tubing was employed. A valve operated by a long lever allowed the air from the pressure tank to pass into a 3-way conduction unit connected with the 3 variators. Intensity was controlled by raising the valve lever to a certain height. The tones sounded for 2 seconds and were stopped by a quick drop of the lever. The variators stood about a meter from the nearest wall, upon which a curtain was hung to minimize reflection. The length of the rubber

*Received for publication by H. P. Weld of the Editorial Board, April 25, 1928.

tubing between the conduction unit on the wall and each variator was the same. Inspection showed that the tones in the chords were approximately equal in intensity and that no lag of any one tone in a combination was noticeable; the 3 variators sounded as a unit.

Four reaction keys in front of *O* operated as many colored lights at the top of the booth. These could be seen by both *O* and *E*. Each chord was given the name of one of the colors, and *O*'s task was to learn to react to every chord by operating the key of the appropriate light.

The 4 combinations sounded were as follows:

Chord	Vibration frequencies	Reaction light
c-e-g	516, 651.7, 775	green (*G*)
B-d-f	489, 580.6, 690.5	blue (*B*)
F-B-d	345.7, 488.2, 580.6	yellow (*Y*)
F-d-a	345.7, 580.6, 870	red (*R*)

The following acted as observers: Professor L. B. Hoisington (H); E. F. Wells, Sage Fellow in Psychology (W); E. R. Moul (M), and the writer (F), graduate students in psychology.[1] M, W, and F had received some musical training, though none in harmony. All had had considerable observational experience.

Procedure 1

In Procedure 1 the *O*'s worked under the following instructions:

"Concentrate on learning the proper reactions to the different tonal combinations which will be presented. A chord will be sounded and a color named by *E*. Respond by pressing the key which operates the light of that color. Each of 3 other chords is to be reacted to in a similar manner. This procedure of naming the reaction color after the chord is given will be continued for a period. Then you will be tested to see if you have learned to react correctly."

In the learning series, *E* sounded each chord and named its color, and *O* pressed the appropriate key. The series of chords was first given 3 times over in the order corresponding to the position of the reaction keys that actuated the lights, viz., *c-e-g* (green), *B-d-f* (blue), *F-B-d* (yellow), and *F-d-a* (red); then each chord was given 3 times over in haphazard order. In every case, *O* made the

[1]W. T. James, graduate student in psychology, acted as *E* for both H and F.

appropriate reaction as the chord was sounded and its color named. Each chord was, therefore, given 6 times. This program was repeated during 3 practice hours. At the end of this practice period every further repetition of the above procedure was followed by a trial series[2] in which *E* sounded the chord but did not name the color corresponding to it, and *O* tried to make the proper reaction.

At the end of the fifth trial series we found that the *O*'s were making no progress in learning to associate the chords with their reaction keys. The practice series was therefore lengthened; the same general program was followed as above, but now every chord in the series was given 4 times over before proceeding to the next in order.

Up to the eighth trial no protocols regarding the reactions made in the trial series were required. From the eighth to the twelfth trials, however, the *O*'s worked under the following additional instructions: "Before you attempt the trial series write a concise statement of the criteria for the recognition of the four chords. After making a given reaction in the series proper, write a brief report on the processes by which the judgment was made. When you have finished your report, turn off the light with which you reacted and the next chord will be sounded." At the end of the twelfth trial this part of the experiment was discontinued.[3]

Results. Quantitative results of the experiment under Procedure 1 are given below in graphic form. This chart covers a period of 5 weeks for all *O*'s and 2 additional weeks for M and W. The abscissae represent the trial series in order; the ordinates, the absolute number of correct reactions. The maximal number of correct reactions in any one trial is 20.

An inspection of the chart shows that the *O*'s failed utterly in their task. They not only did not learn; they did not even improve. This result is not what the doctrine of an "associative mechanism" would lead us to expect. Since few if any bonds of connection seem to have been established, the "law of frequency" was apparently not

[2]In the trial series the stimuli were first presented twice in the regular order (*G, B, Y, R*) and then 3 times in haphazard arrangement. Each chord thus appeared 5 times in a single trial series.

[3]After the twelfth trial H and F began work under Procedure 2 as stated later. M and W, acting as control group, continued as shown in the chart. The chart also shows that the requirement of protocols during trials 8-12 had no appreciable effect upon the learning curve.

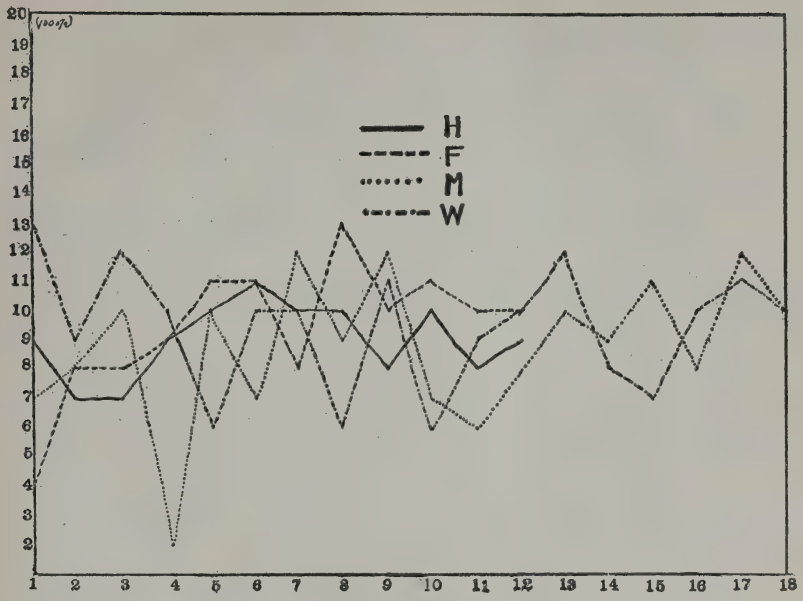

FIGURE 1

LEARNING CURVES FOR FOUR OBSERVERS UNDER PROCEDURE 1

The abscissae represent the trial series and the ordinates the absolute number of correct reactions.

operative. An investigation of the protocols will reveal a reason for this failure. It will be recalled that before the beginning of the eighth trial series the *O*'s were asked to give the criteria for the recognition of the various chords and then, during the trial series, *O* was again requested to give a descriptive report after each reaction. We give as samples the "criteria" and the "reports" of M which are typical of all *O*'s.

"*G* is high and shrill, but has a certain fuzziness low down. The high part gives smoothness. *B* has a fuzziness plus an almost negligible higher tone somewhere else. It is not so well fused. *Y* is low and volumic. Sometimes it is rough. *R* has a minor wail with a sort of 'pulling upward' also. The lower tone is not as low as the *Y*. It is about as full."

M's report in every one of the 20 reactions in the eighth trial series follows:[4]

[4]The first capital letter before each report names the correct color for the chord given, and the second letter indicates the color reaction made.

1. *G-G.* High certainty.
2. *B-B.* Much lower than the former experience—high certainty.
3. *Y-Y.* It has the same downward reference as the other, but not so much so. I am rather certain, however.
4. *R-B.* This has the upness of *G*. It must be *B*—little certainty.
5. *G-G.* The *smoothness* of the total meant *G*. Here I was somehow more passive. The preceding experience was too rough to be like *G*, perhaps it was *Y*.
6. *B-G.* The first experience was one of relaxation. Glow and a feeling of familiarity. The pattern was clear this time, something more than a jumble of aspects. It was as *smooth as the former.*
7. *Y-Y.* Very rough, compared with *G*—high certainty.
8. *R-R.* Not as rough as the former. Might be *B*. I seem to have lost all criteria for the blue.
9. *R-G.* No certainty. It seems like all of them. In the confusion and jumble of aspects and remnants of imaginal context, the only thing that is not hopelessly unrelated is the high tone in the chord. It suggested the highness of *G*.
10. *Y-B.* It isn't the *Y* or the *R* certainly. It somehow has a high character like the one before, but different from it a little. Very uncertain that I reacted right.
11. *R-B.* Can't be *R* or *Y*. Has the unitariness of *G* somewhat.
12. *B-G.* This is more unitary than the preceding. Certainly it is not *B*.
13. *B-B.* Experience did not carry familiarity. It was not as smooth as the *G* nor as rough as the *R*. It was a new experience, and I finally took it to be *B*.
14. *G-R.* More like *B* than any other. It vaguely suggests *R*.
15. *G-R.* Like the former. The pattern is stable now. Good surety.
16. *R-B.* A new experience. I find no pattern to put it into. I can't tell whether it is *G* or not. It is almost as well fused as the last.
17. *B-G.* It was better fused than the former. There was some vague reference to the previous judgment.
18. *G-G.* This may be the *G* and not the former experience. It has the shrill unity of the *G* but the other experience also seemed smooth.
19. *Y-Y.* Rough—might be *R*.
20. *Y-Y.* Rough and very like the other. Better certainty.

A consideration of these two sets of reports shows, in the first place, that they do not agree. It appears from the "criteria" report that M attempted to learn the chords as discrete experiences. *G*, for example, is characterized as high, shrill, and smooth; *Y* as low, volumic, and rough, etc. The reaction report, on the other hand, shows that these individual patterns are either lost or that the chords are "all the same." She begins the reactions by comparing the chords in respect to pitch, and then after the fourth reaction, she drops the aspect of pitch and turns to that of smoothness. At the ninth reaction, in utter confusion, she decides that the pitch

of the experience suggests *G*, and this way of taking all the chords again receives a trial. She tends to discriminate the various chords in terms of some one aspect, but she turns from one aspect to another. Since she frequently compares the chord with the one which immediately preceded it, it is evident that, in the learning series, M made no attempt to relate the chords in terms of a particular aspect. The "criteria" reports reveal, therefore, her method of learning. Her attempt to distinguish each chord by a constellation of aspects was, however, a failure, because the mark of any one chord belonged in greater or less degree to every other chord. Shrill, for example, was one of the characteristics of *G*, but *B* also has a "negligible higher tone," and if this by a shift of attitude should become dominant, *B* would be confused with *G;* the "pulling up" of the *R* might also mean the highness of *G*. *B* has fuzziness, but so also have *G* and *Y*, and if "volumic fullness" is a characteristic of *Y*, it also belongs to *R*.

Since there has been little or no noting of degree of difference in a given aspect, each chord may at times stand for any other. Such a procedure as M tried to follow might be satisfactory if directed and simplified. But instead of taking all 4 chords "volumically," "pitchwise," or in some other way, M allows first one and then another of these several aspects to focalize, and so condition her reaction. Instances of the mechanics of failure reactions are found in the reports above. In 16, for example, M reacted to *R* with *B* because she thought it had a fusion nearly like that of *G*. In 17, when *B* itself was given, it was reacted to as *G*, because it had a better fusion than the sound she had just called *B*. If, however, M had lost her "set" for "fusion" when *B* was given in 17, some other aspect might have been noted, and a different reaction might have been made. We conclude, then, that M carried to the trial series several attitudinal ways of considering the discrete chord. The method of repetition assumed under our general instruction took no account of these variable ways of looking at the thing to be learned, and where the connection between a stimulus and its response is variable, repetition strengthens not any one bond but all that may arise.

While the above discussion has been based solely on the reports of M, the following excerpts from those of the other *O*'s show that their procedure was similar to that of M.

H. There were momentary references to *R*, *G*, and *Y*, but none of these references was stable for any one; there was no particular direction given, and soon there was a lot of unorganized pressure that stood for doubt. I tried to "analyze" the sound experience. I recalled the fairly high pitch which was like that of *G*, but the remembered harshness suggested the *R* fusion. If I had let the thing go on I am quite sure that other references would have come in.

F. There was some reference to a gradient—a scoring of the smoothness of this experience with that of the one before. The smoothness was not like that of the preceding, and I was in doubt as to what reaction to make. Later, before reacting, the focal smoothness became less outstanding . . . . I tried to recall other aspects of the remembered experience.

W. This time the initial set was different. There was less direction to it somehow. At first the coarse harshness of the sound became focal. I don't know what happened to the attitude after this. I am quite sure that it shifted. In that initial sound pattern there was a vague reference to *G*, but this pattern did not develop into a reaction response. Very quickly the coarseness of the sound dropped out and the focal aspect became a clear sharp penetrativeness. The bodily shift which seemed related to this change of focus was complete when I first noted it. Yet there was a suggestion of a "quick turning away from" the coarseness of a moment before.

Two radical changes are suggested as an improvement on the procedure followed in this first part of our experiment: (1) The instructions must condition a specific structure in learning to react appropriately to the 4 chords. It must determine *O* for reacting to some one aspect common in various degrees to all 4 chords. (2) Training must proceed under the dominance of this specific set, must be kept constant, if the individual stimulus-response reactions are to be correctly made.

Procedure 2

In accordance with these suggestions, we next undertook to train our *O*'s in the selection of a particular aspect common to 4 chords and the arrangement of a structural series in terms of this aspect. Since it seemed possible that the 4 chords employed under Procedure 1 did not present any one aspect that could easily be formed in a structure, we decided to select 4 other chords which might readily be arranged in a series which ranged from rough to smooth, and then we formulated an instruction which would require the learning of the sound reactions according to this structure. The chords chosen were as follows: *C-E-G*, smooth; *B-D-F*, less smooth; *C-D-A*, definitely rough; and *C-G-A*, very rough. The *O*'s were at first divided into two groups. M and W, who acted

as controls, continued for some time under the instructions of Procedure 1.[5] H and F, on the other hand, worked under new instructions which were as follows:

"Your reports show that your reaction has been the function of that particular aspect of the stimulus which strikes you at the moment. In order to learn the chords, however, the same aspect must be focal in each experience. If you should recognize the 4 combinations in sequential relationship under one aspect, e. g., roughness or size, your difficulty might be overcome.

"As an aid in developing this kind of structure-function activity, I will present four new chords which easily may be arranged in one gradient structure, e.g., roughness. The 4 will be given in the order from smooth to rough, and corresponding reaction keys are in the same order. A practice period of the usual length will be followed by a trial series. In the trial series set yourself to react to a particular position in the smoothness-roughness structure you have acquired for the total."

As we shall see later, the *O*'s quickly learned to make correct reactions under these conditions. When this was accomplished we returned to the chords employed under Procedure 1, but with the following more general instruction:

"Your task now is to organize the 4 old chords into the same sort of structural whole as that which you have just learned. Since the structure is not provided for you, it will be necessary to consider the several aspects presented by each chord before deciding which is most adequately differential. As the tones are presented and named, attempt to grade and place them on the scale suggested by the differentia of your choosing. Then name the order in which the suggested chords appear in this structure. The keys will be arranged to suit this structure and this order will be repeated until you feel that the set is so stabilized that the experience will always appear with the one aspect as focal. In the trial series which will follow you are to take every experience in this definite way. If you lose the set and the experience comes as one not fitting into your scheme, react with the white light which will indicate that the experience is "new."[6]

After the first trial series under this procedure, in which every *O* had shown good progress, reports were called for, as follows:

"Before the beginning of the trial series, write a brief report of the

[5]See chart, Procedure 1, trials 12-18.

[6]The key for the white light was placed at a short distance from the constellation of 4 keys which operated the colored lights. The chords reacted to by this light were again presented later in the series until they were reacted to as familiar. The same opportunity was accorded to the control group (trials 12-18 inclusive). The trial series was the same in number and arrangement as that given in Procedure 1.

attitude or set which is present at the moment. Upon finishing the trial series, give a general report of the judgments which you have made. Mention any shifts in the initial attitude which may have occurred. Report the progress by which the judgments or reactions are now made."

The first trial series was run at the same hour that the attempt to "structure" the 4 chords was made, and was repeated but 4 times thereafter. With the success of H and F, the procedure called for a repetition of the new method by the control group. In the tabulated results shown below it should be noted that H and F completed the experiment before M and W.

Results. Under this procedure learning occurs rapidly. The total integrated pattern presents itself at once, and becomes more "precise" with repetition. With the "simple" problem, 3 *O*'s, H, F, and W, gave the right reactions *in toto* the first time they tried a series of 20 in haphazard order. M experienced difficulty in the first trial, where 16 out of 20 were reacted to properly, but at the second presentation of the series, she also gave a perfect set of reactions.

The following results were obtained with the same chords that had given so much difficulty under the first procedure:

Trial no.	Percentage judged right by *O*			
	H	F	W	M
1	90	85	80	75
2	90	100	85	75
3	100	100	85	80
4	100	100	100	80
5	100	100	100	100

These results become significant when we compare them with those obtained under Procedure 1. There the highest percentage of correct reactions in any trial was 65 (W-trial 1, F-trial 8) and the highest individual average for all trials was only 45.4 (F).

In considering this remarkable improvement, it is worth noting that no *O* was aware of the particular aspect which any of the others was using. Each, working independendently, chose a gradient which was to him adequately differential for the 4 chords, and then assigned each of these to a relative place in a structure. The several aspects chosen, together with the structural patterns of each, are given below. The fact that the effort of each *O* was

successful shows that achievement is a function of the method involved, and not of the particular aspect chosen.

Observer	Focal aspect chosen	Position of chord in the structure
H	size or volume	*Y, B, R, G*
F	degree of dissonance	*G, B, Y, R*
M	degree of dissonance	*G, B, Y, R*
W	texture	*G, B, R, Y*

While our inference from the difficulties involved in our first procedure is sustained, several questions yet remain as regards the nature and function of *O*'s attitude under Procedure 2.

1) *The relation of attitude to structure.* The general set or attitude is a kinaesthetic pattern which carries the instruction. This precedes every experience in the trial series and anticipates an auditory experience. There is also a definite reference to the task. The set is specific in the sense that it "looks for" experience to develop in a certain way; it is, for example, a set for "size," or "dissonance," or some other aspect in every tonal experience that comes. In the training period, therefore, all chords come with some one aspect dominant, and in successive experiments the chords are arranged in a scale. The 4 chords thus formed owe their "togetherness" to the similarity of their dominant aspect. Since all are different degrees of the same thing, each chord occupies a relative position in the structure or scale. In the trial series, *O* does not consider all the positions on the scale and then single out that one which the present chord most resembles, but he immediately reacts to it as a position in the scale. As the structure becomes more stable, the bodily set which conditions the development of the structure becomes less active and more passive. The sounds, however, still carry the same aspect focal as under the more active determination.

The above statements find their basis in the following reports:

H. The set (initial trial) is for size of the auditory experience. Experience is so "pre-formed" that only the size aspect of the sound becomes clear or is noted. Also I believe that the stepwise nature of this size is more or less predetermined but the particular step comes in after the sound experience. The set today (fifth trial) is much more passive than when I first set out actively to acquire a particular way of doing the thing. The set today is to test the sounds by the remembered scale rather than by taking them more actively to get that scale. The set for size seems very stable now.

W. The set immediately preceding each sound experience was a set for sound-texture. When the sound came, there arose a bodily pattern which meant a certain degree of "density-extent" (texture). The general set which arises preceding the trial series is of the same sort. It bears a vague reference to the task and some meaningful hint that I am to look for the texture of each experience.

M. The initial set is rather active over against the experiences that are to come. The attitude is characterized by a strain-like pattern of expectancy in a certain direction. As the sound becomes focal on this bodily pattern, it means a certain degree of fusion or "spread-outness." The pattern of the completely fused experience is compact-like. The bodily background or *set* comes to spread out more with the less well-fused chords.

F. The general bodily set is of highly strain-like character with little looseness to it. There is a definiteness to the pattern of expectance—a tuning or specialized determination that refers to what is to come—the experiences all tend to be heard under the dominance of the same initial set of attitude, and their foci develop in accordance with it. The experiences come "dissonated"—that is, the specific patterns seem to come at some particular place in the scale which I have structured. I do not have to consider all possible positions.

2) *The stability of the attitudes.* Once the *O* has taken up a specific set over against the tonal experiences, his attitude is for the most part stable and uniform. If the attitude is lost, the sound will come as new; since learning took place with one aspect dominant, another kind of focality would make for a new experience. The structural pattern also is strengthened as the connection between focal aspect and the determination attitude becomes habitual. There is a progressive decrease of "new" reactions, and the attitude itself becomes an habitual one. The table below shows the number of times that *O*'s attitude shifted. *O*, in these cases, reacted with the "white" light, thereby indicating that he had lost his determinate set. These reactions are not counted in the percentages of correct reactions given at the beginning of this section.

No. of Times *O*'s Attitude Shifted

Trial no.	H	F	W	M
1	5	3	4	7
2	4	2	2	7
3	2	0	0	5
4	0	1	2	2
5	1	0	0	0

3) *Causes of shift in attitude.* This problem, we feel, can not be adequately dealt with here, although there are many hints given in the reports. In the first trial under the new procedure *O*'s

attitude shifted very easily, and even in the later trials it still tended to shift with any distracting influence. The difficulty of M and W to maintain a constant attitude may be due to the distractive influence of the longer continued work under Procedure 1; H and F have also mentioned the previous work as a distinct hindrance. At times the sound came as "not quite right"—as not fitting in with the structure for which *O* was set. While this has not been adequately explained, we do know that such an experience upsets *O*, and leads him to turn to some other aspect of the sound; often it seems that this new aspect of the experience was already determined when the former aspect came as not fitting. For a time after any such experience as we have described, *O* found it difficult to take the original set "cleanly."

H. The initial set carried without noticeable disturbance till toward the middle of the series. Then a sound that did not fit in "size" led me to turn to another aspect of it. This did not help. After that for a time I found it more difficult to take and keep the "size set" alone—I feel that the previous work on these chords is a very distinct hindrance. It is very easy to turn from size to some aspect which has probably been focal in the past. The size first comes as a little off, and then another aspect bobs up.

W. Once, when *R* came, the attitude shifted momentarily, and the pitch of the sound became focal; the experience was in this sense "new," yet it was vaguely familiar as *G*. When the next sound was presented I reacted to it as the texture of *R*. Then I realized that the former had been *R* also.

M. It seems that no matter how disposed I am for the "fusion" aspect, my attitude shifts when *G* comes. The pitch comes out as dominant. Perhaps it is not always the *G* that shifts the attitude. After an experience of this kind, the "fusion" set isn't pure, but is slightly tinged by a hangover from the "pitch" attitude.

F. Once the experience came as "new." Here the initial set was not a specifically "dissonate" one. It was not sharply limited or bounded. I took to questioning the sound. The attitude tends to fog up the pattern as it comes and allows a different aspect to become focal.

4) *The integrating of focal aspect with reaction judgments.* The reaction seems to be inherent to the sound pattern. The focal aspect of the sound comes as a particular step in the graded scale and is represented by a specific colored light. Every colored light, moreover, was associated to a particular reaction key, and the 4 reaction keys were arranged in an ordered series corresponding to the structure of the chords. In general, when the sound came to *O*, it not only fell into its place in the structure but its color name also appeared and the motor movement belonging to the color followed automatically. In some cases at the latter part of the experiment, the color reference dropped out and the reaction followed immediately

upon the structural localization of the sound. We may, therefore, regard the chords and the reactions to them as together constituting a total structure.

W. This specific sound texture pattern carried with it a reference to its color. The reaction grows out of the sound texture pattern. The color reference comes out clearer, the response follows, and the bodily set that carried the sound texture pattern is replaced by something like relief.

H. The set was for size, but non-specifying. As soon as the sound came its size was focal, and if this size was somewhat familiar the reaction followed immediately.

F. The focal aspect first contained a vague reference to a color. The color then became clearer and stood for the sound. The bodily strain pattern finally became a bit more compact and the reaction of pressing a key came automatically.

M. The process of reacting consists in a feel of "spread" for the chords. If a familiar spread comes it has a color reference bound up with it incipiently. When the color comes out the reaction follows.

Summary

Under Procedure 1, the numerous attitudes or sets permitted by the general instructions functioned against the formation of right associates. Due to the various attitudes of *O*, several aspects of the sound experience could arise from the same stimulus. So far as we went with this procedure, we obtained no evidence of an associative mechanism per se. While there exists the possibility that the complexity of the sounds did not permit of a simple linkage of dominant aspects with the color reactions frequently enough to condition learning, our results show that learning by repetition alone is, to say the least, uneconomical. We found that the learning of the material was greatly facilitated by the operation of a particular attitude or way of taking the chords. This "insight," resulting from the instruction, exhibited a determining influence over the items to be associated. These items, the chords and color reactions, were organized in a total structure by a process which grew out of the determination. One particular aspect common to the 4 chords became focal and fell in a series every term of which was linked with a particular color reaction. In other words, the 4 reactions were incorporated in the structural formation. Since, as we have said, the organization of the structure did not occur in the absence of a specific determination, we may conclude that attitude or "insight" is a primary condition of the structural formation.

Conclusions

From the results of our study, we may conclude:

1) In the learning of material that is complex (in the sense that it has many aspects common to all its members), a method that implies mere repetition is uneconomical.

2) The economical learning of such material requires an attitude or "insight" which conditions a structural formation of the material to be learned.

3) Our experiments have shown the advantage of singling out one aspect that is common to all the members of the material and of attaching structural significance to it.

4) An attitude or "insight," such as the one we have mentioned under (2) that will function in the way we have described under (3), may be directly conditioned by a proper *Aufgabe* or instruction.

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LA RELATION ENTRE L'ATTITUDE ET L'APPRENTISSAGE

(Résumé)

On a demandé à quatre observateurs d'associer chacun de quatre accords musicaux avec une certaine lumière de couleur, mise en action au moyen de presser un manipulateur télégraphique. Dans une série à apprendre, basée sur la supposition que la répétition est le facteur dominant de l'apprentissage, l'expérimentateur a sonné un accord et a nommé la lumière de couleur à associer. Après avoir sonné chaque accord de cette manière 36 fois, on a présenté des séries de tests, et désormais chaque période d'expérience a compris une série à apprendre et une série de tests. A la fin de la douzième série de tests, il est devenu évident qu'aucun des observateurs ne faisait de progrès dans l'apprentissage. (Voir la table p. 101). Les résultats ont indiqué que cet insuccès a résulté de ce qu'aucun seul aspect d'un accord n'a servi de moyen d'identification et d'apprentissage. Il a semblé probable, pour cette raison, que les observateurs pourraient apprendre plus facilement, si on leur demandait de choisir un seul trait caractéristique qui se trouvait dans les quatre accords mais non dans le même degré. Par conséquent on a répété les expériences avec une instruction nouvelle qui a fait que les observateurs organisent les quatre accords dans une "structure" dans le sens de la *Gestalttheorie*. Avec cette instruction tous les quatre observateurs ont subi la cinquième série de tests sans une seule erreur. (Voir table p. 106).

L'auteur conclut qu'une méthode qui implique la répétition seule n'est pas économique dans l'apprentissage d'une matière complexe (dans le sens qu'elle a beaucoup d'aspects trouvés dans toutes ses parties); que l'apprentissage économique de telle matière exige une attitude qui conditionne une formation structurale de la matière; que les expériences montrent l'avantage de

choisir et d'attacher une signifiance structurale à un seul aspect trouvé dans toutes les choses à apprendre; et que l'attitude nécessaire peut être directement conditionnée par la *Aufgabe* ou instruction nécessaire.

FREEMAN

EINFLUSS DER EINSTELLUNG AUF DAS LERNEN

(Referat)

Man forderte vier Vpn. auf jeden von vier Accorden mit einem besonderen farbigen Licht zu verbinden, das durch einen Telegraphen Schlüssel kontrolliert wurde. In einer Serie basiert auf die Annahme, dass Wiederholung die Hauptbedingung beim Lernen sei, schlug der VI. einen Accord an und nannte das farbige Licht das damit zu verbinden war. Nachdem jeder Accord auf diese Weise 36 Mal angegeben worden war, gab man Test Serien, und danach bestand jedes Experiment aus einer Test Serie und einer Lern Serie. Am Ende der zwölften Test Serie wurde es klar, dass keine der Vpn. Fortschritte im Lernen machte (Siehe Ab. S. 101). Die Protokolle deuteten an, dass dieser Misserfolg dadurch verursacht wurde, dass kein einziger der verschiedenen charakteristischen Züge eines Accords zur Identifizierung und zum Erlernen diente. Es schien daher wahrscheinlich, dass wenn man die Vpn. anleitete einen charakteristischen Zug zu wählen, der in verschiedenem Masse allen vier Accorden eigen war, dies das Erlernen bedeutend erleichtern würde. Das Experiment wurde daher wiederholt, doch mit der neuen Anweisung die vier Accorde zu einem "Ganzen" zucamnsenzufügen im Sinne der *Gestalttheorie*. Nach dieser Anweisung bestanden alle vier Vpn. den fünften Test ohne einen einzigen Fehler (Siehe Tafel, S. 106).

Der Verfasser schliesst daraus dass beim Erlernen von verwickeltem Material (in dem Sinn dass viele charakterische Züge allen seinen Teilen eigen sind) eine Methode die nur Wiederholung anwendet, zeitraubend ist; dass das zeitersparende Erlernen solchen Materials eine Einstellung erfordert die ein logisches Zusammenfügen des Materials bedingt; dass die Experimente beweisen wie vorteilhaft es ist einen Zug auszuwählen der allen Items die zu erlernen sind eigen ist, und ihm eine structurelle Bedeutung beizulegen; und dass die erforderliche Einstellung unmittelbar bedingt werden kann durch die geeignete *Aufgabe* oder Anweisung.

FREEMAN

A COMPARATIVE STUDY OF GROUPS OF HIGH AND LOW INTELLIGENCE IN LEARNING A MAZE*

From the Psychological Laboratory of McGill University

K. W. Spence and S. Townsend

The study herein presented is a report of a half-term's work in an introductory experimental psychology course at McGill University. The study was undertaken primarily with the view of gaining some knowledge of the technique and problems of experimental psychology, and not with the hope of being able to make any great contribution to psychology. From this point of view the study has been a decided success, for we feel that we have gained at least some appreciation of the problems and methods of experimental psychology.

Fully realizing our lack of experience, we hesitated considerably in making any definite conclusions, but at the same time we feel that the individual records we obtained are valid and not the result of varying experimental conditions or lack of uniformity in procedure.

The Problem

The purpose of the study was to determine whether there is any relation between general intelligence, as indicated by an intelligence test score, and ability in motor learning, as indicated by the learning record on a maze. Incidentally, any other interesting or significant findings are also included in the report. The maze problem, requiring as it does the building up of a reaction pattern largely in terms of muscular control in the mastering of it, has come to be employed extensively in the study of motor learning in the laboratory, and a large number of studies in maze learning have been made.

Two studies of the problems of maze learning that contain sections on the questions with which we are here concerned have been reported by Hunter (5) and Warden (9). Hunter in his study found a positive relationship between performance on a maze and the Otis test. Table 1*a* shows the correlations he found.

Warden also made a comparison between the learning record on a

*Recommended by Chester E. Kellogg and received for publication by Carl Murchison of the Editorial Board, November 26, 1928.

TABLE 1*a*
CORRELATIONS REPORTED BY HUNTER*

	Maze Record		
	Total time	Total trials	Total errors
Otis Test	—.45±.08	—.17±.094	—.34±.087

*The relationship is positive notwithstanding the negative correlations, for a good maze performance means a low time, trial and error record, while a good intelligence score means a high score. Thus a positive relation between the numerical records would give a minus correlation coefficient.

TABLE 1*b*
CORRELATIONS REPORTED BY WARDEN

No. of subjects	Alpha-Maze	Maze-Thorndike	Alpha-Thorndike
40	.37±.091	.30±.097	.59±.068

maze and intelligence test scores. He gives correlations between speed in learning a maze and intelligence scores as shown in Table 1*b*.

Thus from these 2 reports there appeared to be some relation between the 2 abilities. Much depended, however, they concluded, upon the form or pattern of the maze employed. Hunter, in comparing the maze records and intelligence, found that the more complex habits (the more difficult mazes) offered the lower correlations. He felt that the simpler mazes gave less opportunity for chance to affect the records and a greater amount of control over the subjects so that the data with them would be more dependable. The factor of chance, it has been found, is very great in mastering the maze problem, varying with the pattern of the maze. Heron (3) in a study of the problem concluded that in no cases was individual ability found to be more influential than chance. It is not, however, our purpose here to study the comparative influence of these 2 factors in maze learning, but rather to ascertain whether there is any relation between intelligence test scores and maze performance. Do individuals with a high intelligence score do well on the maze in comparison with individuals who obtain low scores on intelligence tests? Or is the opposite the case?

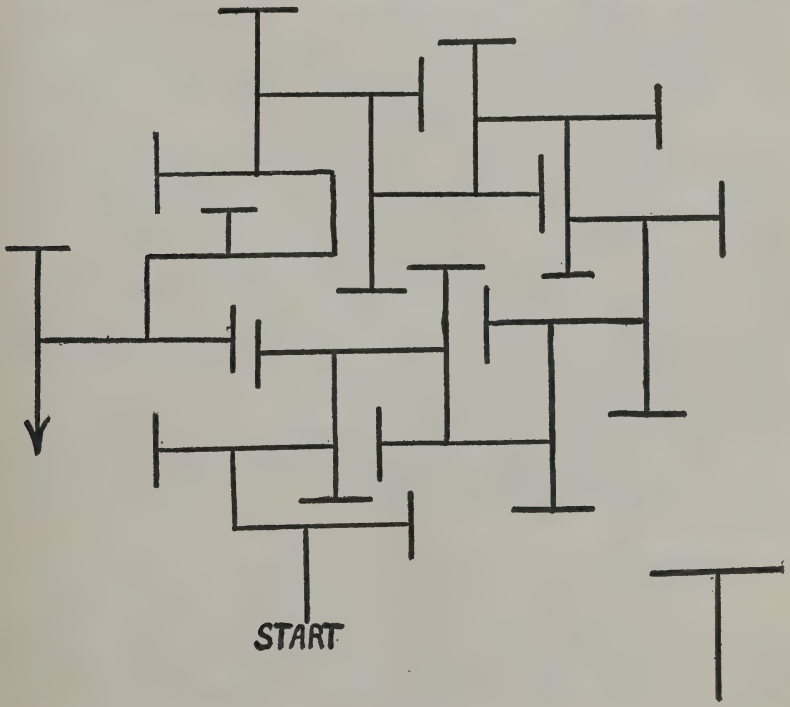
For several reasons, chief of which was the lack of time necessary to handle enough subjects, the correlation method of attacking the problem, as the 2 studies referred to above employed, was not adopted. Instead, a comparison of the performances in a maze of

a group of individuals who scored high on an intelligence test with a group of individuals who scored low on the same test was made. As the individuals were selected (so far as was known) on no other basis than the factors that make for a high intelligence score, any differences in the performances of the 2 groups might be referred in some degree at least to these factors.

Apparatus and Method of Procedure

Apparatus

The maze used in this study was one constructed in the laboratory workshop by ourselves. It was a high-relief finger maze made after the manner suggested by Walter Miles (6). It was constructed of No. 14 copper wire (1/16 inch in diameter) bent into long square-cornered staples of suitable lengths for the pattern desired and these were fastened on to a wooden panel. The panel was 18 inches square, 12 square inches of which was covered by the pattern. Or-



The Maze

dinary rubber feet were fastened at the corners in order to make the panel more stationary while in use. The turns or choices were all right-angled ones, and the distance between choices was never more than 35 mm.

The blindfold equipment consisted of a cheap pair of automobile goggles, the glasses of which were covered by surgeon's adhesive tape, applied to the inside. This blindfold is much more efficient than just a handkerchief tied over the eyes, for the subject is able to keep his eyes open and thus be very comfortable even for a long session of work.

Method of Procedure

The subject was seated at a table on which was placed the maze (covered). The subject was instructed as to the nature of the response to be made. The problem was stated to be "learning to go from the entrance to the goal without making any false turns into blind alleys for three trials in succession." After the blindfold had been adjusted and the maze uncovered, the subject was given the following verbal instructions: "There is one place in the maze where you will run into an obstacle (the experimenter's pen). The object for each trial is for you to follow along the wire path exploring different paths in an attempt to reach this goal with as few moves as possible. There is but one correct path from the beginning of the maze to the goal and this route is constant. In addition to the correct path there are a number of blind alleys (where you will run off the wire). An error will be counted for any entrance into a blind alley and for any attempt at retracing the correct path. That is, you will not be allowed to retrace; any retracing started will be stopped, but an error will be counted. Your object is to learn to follow the correct path without making any errors. The maze will be considered learned when you have made 3 perfect runs in succession. The time of each trial is taken, but remember the experiment is completed only after 3 successful trials have been made in succession."

The hand not used in tracing was then placed in a resting position at its corner, and the index finger of the other hand, having been given a sample experience of what the maze was like (the nature of choices and blinds) was placed at the entrance of the maze, and the signal was given to go. Ten trials were given the first day. Two

days later the subject was given as many trials as were necessary to master the problem, a short rest being given if too many were required. As the subjects were not trained in introspection no introspective records were taken.

Method of Recording

Records were made of each individual's performance. The trials taken, the number of errors on each trial, the time for each trial were all carefully noted and recorded.

The possible errors were of 2 types: (1) any entrance into a blind alley, (2) any retracing of the correct path. The data were then tabulated in such a way as to make possible both group and individual treatment of the results. Not only is group behavior a truer criterion upon which to base conclusions as to the learning ability which would hold for the class, but idiosyncrasies of individuals are lost in the totals, and chance happenings have less weight.

Subjects

The subjects were taken from upper classmen who had taken, or at the time were taking, the elementery course in psychology. No subject had ever had any previous experience with mazes.

Unfortunately, due to lack of time, it was not possible to have a very large number of subjects. Thus only 20 students were experimented with, 10 who had scored very high in the Thurstone Intelligence Test and 10 who had scored very low in the same test. This test is given to all students who take the elementary course in psychology. Thus there were obtained 2 very distinct groups, Group A with high scores and Group B with low scores. Group A scores averaged 31.6, ranging from 27 to 36; and Group B, 14.3, ranging from 10 to 17; while the average of the total group from which these individuals were selected was 22+. To qualify for the high group it was necessary to have at least 27, that is, 5 more than the mean of the total group; and to qualify for the low group, 17, that is, 5 less than the mean of the total group.

Results

Comparison of the Two Groups

In this section we will first analyze the performances of the 2 groups, especially noting any differences between them; later we will take up the important problem of the reliability of these results.

TABLE 2
THURSTONE INTELLIGENCE TEST SCORES

Group A		Group B	
Name	Score	Name	Score
1. Gu.	31	1. Dia.	15
2. Hu.	35	2. Nos.	14
3. Est.	31	3. Scha.	16
4. Kimp.	31	4. Ro.	10
5. Nor.	27	5. Rho.	14
6. Will.	33	6. Dou.	17
7. Clar.	36	7. Bor.	13
8. Shel.	32	8. Walli.	15
9. Ses.	28	9. Fitzu.	15
10. Wi.	32	10. Har.	13
Av.	31.6	Av.	14.3
Range	27-36	Range	10-17

TABLE 3
RECORD OF GROUP A

Name	Trials	Errors		Time (in sec.)	
		Total	Av. per trial	Total	Av. per trial
1. Gu.	18	70	3.8	993	55
2. Hu.	20	90	4.5	973	49
3. Est.	21	73	3.4	913	43
4. Kimp.	23	72	3.1	870	38
5. Nor.	23	94	4.0	1282	56
6. Will.	22	105	4.7	924	42
7. Clar.	24	69	2.8	1078	45
8. Shel.	25	103	4.1	744	30
9. Ses.	38	195	5.1	1358	36
10. Wi.	18	92	5.1	787	44
Avs.	23.2	96.3	4.06	992.2″	43.8″

The records of the 2 groups are given in Tables 3 and 4. The numerical data in these tables, giving individual error, time, and trial records, as well as the average records for both groups, invite numerous comparisons. It will be seen that in all comparisons of the scores of the 2 groups there is a decided superiority shown by Group A, that group of individuals which scored high on the Thurstone test. The average number of trials taken by the Group-A individuals was 23.2, ranging from 18 to 38, as opposed to 40.3 by B individuals, who ranged from 16 to 66. The average number of

TABLE 4
RECORD OF GROUP B

Name	Trials	Errors		Time (in sec.)	
		Total	Av. per trial	Total	Av. per trial
1. Dia.	19	104	5.4	780	41
2. Nos.	31	120	3.8	1439	46
3. Scha.	33	176	5.3	1275	39
4. Ro.	40	271	6.8	2695	67
5. Rho.	54	301	5.7	2593	48
6. Dou.	56	178	3.2	2245	40
7. Bor.	16	79	4.9	904	57
8. Walli.	35	166	4.7	1615	46
9. Fitzu.	53	127	2.4	1573	30
10. Har.	66	295	4.5	2528	38
Avs.	40.3	181.7	4.67	1764.7"	45.2"

total errors of the A group was 96.3 (range 69-195), compared with Group B's average of 181.7 (range 79-301); and the average total time of Group A was 992.2 seconds (range 744-1358), while B individuals took on the average 1764.2 sec. with a range of 780-2695. Particularly noteworthy is the vast difference between the 2 groups, Group A's record being nearly twice as good as that of Group B in all 3 measurements. There is some overlapping, of course, but it is very slight. Only 2 Group-B individuals scored better than the averages of Group A, while only 1 Group-A individual did worse than Group B averages, and that was only in the case of total errors. At any rate, there is a considerable difference in the performances of the 2 groups in the maze test, and it would seem that the factors that make for a high intelligence score also make for a high performance in the maze test.

We must now consider the reliability of our results in order that we may see just what significance may be given them. Also there must be considered the reliability of the maze test itself as a measuring instrument. Unfortunately, as has been mentioned above, our time was exceedingly limited, and it was not possible either to obtain a larger number of cases or employ other differently patterned mazes.

The usual measures of reliability were all calculated (i.e., $P.E._{mean}$, $P.E._{diff.}$, and also the critical ratio, I). The small number of cases necessarily limits their value, of course, and some caution must be observed in interpreting results secured from them.

An attempt was made also to find the reliability of the maze test used, for the 2 groups by correlating the errors (time) made on the even trials with the number of errors (time) made on the odd trials by each individual. Thus, 2 correlation coefficients were obtained for each group. These coefficients were exceedingly high, ranging from +.93 to +.98. Just what significance may be attached to these coefficients it is difficult to say. Admittedly they do not measure the consistency of the maze test in the same manner as a "reliability coefficient" calculated by correlating the scores made on a test, i.e., intelligence test, with the scores made on the same or a duplicate test by the same group. They do show, however, the reliability of the learning process itself in the maze, that is, that once the learning process is started it is consistent, chance factors not playing an important part at all. Or to state it in another way, however much chance may have influenced the record on the first trial, the records of the following trial, on the whole, are consistent, being determined by individual ability and not by chance. These "coefficients of reliability" are shown in Table 5.

TABLE 5

Criterion	Group A	Group B
Errors	.94±.02	.98±.01
Time	.93±.03	.97±.01

There has been a considerable difference of opinion shown as to the reliability of the maze tests in measuring differences of ability. Heron (3), as has already been mentioned, concluded from a study he made that chance was a very large factor in the production of scores and that in no maze could it be said that individual ability constantly plays a dominant part (a conclusion diametrically opposed to our findings). "Consequently," he further concludes, "the difference between the averages of two groups of individuals, one of which ranks higher than the other in one maze, must be taken as an absolute measure of difference in ability of the two groups." Similarly, Hunter (4) states that maze records are highly inaccurate measures of individual differences in ability, and hence this problem cannot be regarded as a reliable device for measurement of such differences. This statement is questionable so far as the type of maze here employed is concerned at least. But even admitting that indi-

vidual records are considerably determined by chance, Hunter's proposition that maze records cannot be utilized in making group comparisons because these records are not accurate measures of individual differences of ability does not follow, as has been pointed out by Carr (2). The reliability coefficient necessary for tests in making group comparisons is not nearly so high as that required by measuring individual differences in ability. This question is, of course, more properly a problem for statistics.

As to the question of the reliability of the present maze, which must necessarily be known, according to Hunter, before any group differences found can be accepted, we first refer the reader to Tryon's article on "The Effect of the Unreliability of Measurement on the Difference between Groups" (8). He has shown that, when a difference between 2 groups is empirically found, that difference is more significant in proportion as the measuring device *is less reliable.* Or in other words, he has shown that under the lowest condition of reliability the difference is at least as significant as is shown by the ordinary "critical ratio" formula ($I = \frac{Diff.}{P.E._{diff.}}$) and becomes more significant when "corrected" for the unreliability of the test. "The very fact," he states, "that a significant difference is empirically found is a sure sign of some fundamental reliability . . ."

How significant, then, is this difference between the averages of the 2 groups? We first worked out the significance of our results with the ordinary $\frac{Diff.}{P.E._{diff.}}$ formula, neglecting to take into account the reliability of our maze test; but in the light of Tryon's article we can see that any unreliability of the test would have only the effect of making the difference even more significant. Table 6 shows how significant the difference between the 2 groups is, and, as we see, it is very high, the average of the 3 "critical ratios" being 4.8. From such a large and highly significant difference between the 2 groups one might be led to conclude that the maze used was highly reliable. If we consider the correlation coefficients of Table 5 as measuring the reliability of the maze test, the "critical ratio" when corrected becomes 4.77 instead of 4.5, and similarly the critical ratio for time is higher.

Thus we see that the difference between the 2 groups in their

TABLE 6

COMPARISON OF THE RELIABILITY OF THE TWO GROUPS

	Group A		Group B		Difference in favor of Group A		
	Mean	$P.E._{mean}$	Mean	$P.E._{mean}$	Diff.	$P.E._{diff.}$	$I = \frac{Diff.}{P.E._{diff.}}$
1. Thurstone score	31.6	.55	14.3	.39			
1. Total trials	23.2	1.15	40.3	3.35	17.1	3.54	4.8
2. Total errors	96.3	7.5	181.7	16.38	85.4	18.01	4.5
3. Total time	992.2″	40.1″	1764.7″	140.97″	772.5″	146.50″	5.3

performance on the maze was very large and, even considering the small groups, highly significant. Group A, on the average, did much better than Group B. And it would appear from these results that the relationship between the Thurstone score and the quality of the maze performance is quite high.

A comparison of the variability of the A and B individuals as groups is given in Table 7. As will be seen, Group A was not nearly

TABLE 7

VARIABILITY OF THE TWO GROUPS

	Group A		Group B		
	Standard deviation	% of variability $\frac{\sigma}{AM} \times 100$	Standard deviation	% of variability $\frac{\sigma}{AM} \times 100$	% of variability of A / % of variability of B
1. Thurstone score	2.4	7.6%	1.83	12.8%	59.3%
1. Total trials	5.41	23.3%	15.71	38.9%	59.8%
2. Total errors	35.35	36.7%	76.9	42.4%	86.5%
3. Total time	188″	18.8%	661.8″	37.5%	50.2%

so variable as Group B. Group A was 54.4% as variable as Group B in total time, 59.5% as variable in total trials, and 86.5% as variable in total errors. Also from this table it can be seen that the greatest variability was shown, in the case of both groups, in total errors, the least in total time.

Correlations

Although correlations with such a highly selected group are not of very great value, being liable to considerable error, it was felt that they would be of interest as indicating in a different manner the relationship between the Thurstone scores and maze performance. Table 9 gives the correlation between the ranks of individuals in the Thurstone Test and their rank in the maze test. As will be seen, the correlations are positive and fairly high, ranging from .46 to .63. Of course, these coefficients are not accurate indications of the extent of correlation, but they do indicate the presence of a decidedly positive relationship between the 2 tests.

Table 10 gives the coefficients between the actual scores in each test. These coefficients more correctly indicate the extent of the

TABLE 8

Rank	Thurstone score	Maze results: Trials	Errors	Time
1	A-7	*B-7*	A-7	A-8
2	A-2	A-1	A-1	*B-1*
3	A-6	A-10 *	A-4	A-10
4	A-10	*B-1*	A-3	A-4
5	A-8 *	A-2	*B-7*	*B-7*
6	A-1	A-3	A-2	A-3
7	A-3 *	A-6	A-10	A-6
8	A-4	A-4	A-5	A-2
9	A-9	A-5 *	A-8	A-1
10	A-5	A-7	*B-1*	A-7
11	*B-6*	A-8	A-6	*B-3*
12	*B-3*	*B-2*	*B-2*	A-5
13	*B-1*	*B-3*	*B-9*	A-9
14	*B-8* *	*B-8*	*B-8*	*B-2*
15	*B-9*	A-9	*B-3*	*B-9*
16	*B-2*	*B-4*	*B-6*	*B-8*
17	*B-5*	*B-9*	A-9	*B-6*
18	*B-7*	*B-5*	*B-4*	*B-10*
19	*B-10*	*B-6*	*B-10*	*B-5*
20	*B-4*	*B-10*	*B-5*	*B-4*

*Tied.

TABLE 9

r=ranks*	Trials	Errors	Time	All
Thurstone score	.46±.12	.63±.095	.60±.10	.50±.12

*Calculated by the rank-difference method.

TABLE 10

CORRELATION BETWEEN THURSTONE AND MAZE RECORDS*

Group	Thurstone-Maze Trials	Thurstone-Maze Errors	Thurstone-Maze Time
1. Total Group	—.57±.11	—.64±.09	—.66±.08
2. Group A	—.41±.18	—.18±.21	—.52±.15
3. Group B	+.09±.21	—.38±.19	—.30±.20

*Positive relationship is denoted by a minus coefficient.

correlation, although they are by no means very accurate, due to the fact that we are dealing with a highly selected group. Row 1 gives the correlation with all the individuals taken as one group. It will be seen that they are even higher than those obtained by the rank-difference method. Row 2 gives the correlation between Thurstone and Maze scores in Group A, and Row 3 the correlation in Group B. It will be seen that, in every case but one, the relationship is positive (a minus coefficient). The correlation coefficients for the separate groups (A and B) are not very valuable indications of the relationship because of the small size of the groups and of the small differences between the Thurstone scores of each individual in these groups.

Perhaps of more significance than these correlation coefficients is the evidence of Table 8. Here the individuals are ranked in order (the most efficient being ranked 1, etc.) for each criterion. It shows very clearly the superiority of Group-A individuals who predominate at the high end of the table. Only 2 Group-B individuals, B-1 and B-7, were able to hold their own very successfully with the Group-A individuals. Also it shows that only 1 Group-A individual did poorly on the maze, namely A-9.

Although it is not, probably, a very good procedure to attempt to explain away all the inconsistencies of the results of any experiment, it is nevertheless interesting to note in connection with the 2 Group-B individuals who did so well, B-1 and B-7, that one is at present an honor student in another department and his work is pretty generally above the average, despite his not being a very diligent student. The other is doing very good work in the elementary psychology course and is judged by one of the instructors in the department, for whom he also was a subject, as being above the average in intelligence. Also it is very interesting to note that both of these individuals are rather highly emotional. This might

have influenced their intelligence score for the worse. However, as we stated above, we are not attempting to explain away these discrepancies but are merely pointing out some interesting and perhaps important points concerned with them. Our results, as they stand, show pretty conclusive evidence of a group distinction in learning the maze.

Learning Curves

Several learning curves are given for purposes of comparing the 2 groups; we have not attempted by any means to make a detailed analysis of them. Figure 1*a* shows the elimination of errors by the two groups. It will be seen that the curves are very similar to each other, both showing a rapid initial descent and numerous steeples. The learning process is evidently essentially the same in the 2 groups, but Group A's learning is faster and also more permanent.

Figure 1*b* gives a comparison of the rate of elimination of errors. The comparison is facilitated by a translation of the gross values into percentage terms. By dividing the value of each trial by the value of the first total, a series of percentage results is secured decreasing from 100 to 0. The value for any trial indicates the percentage of the initial value yet to be eliminated. In con-

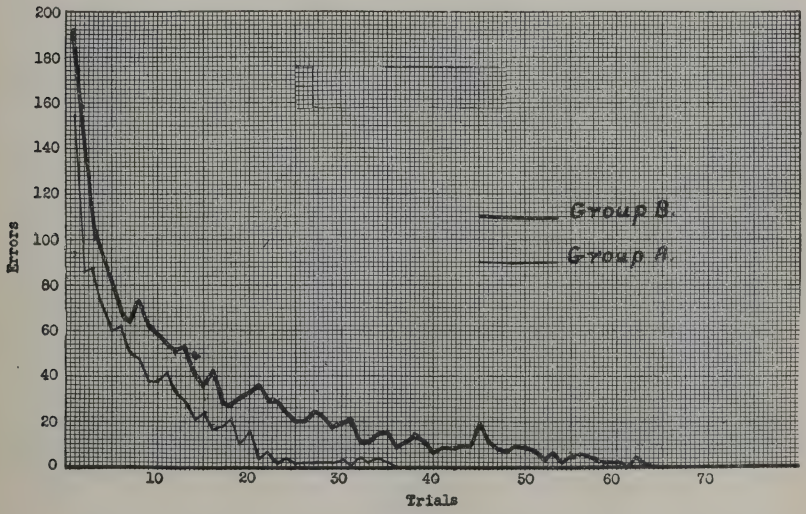


FIGURE 1*a*

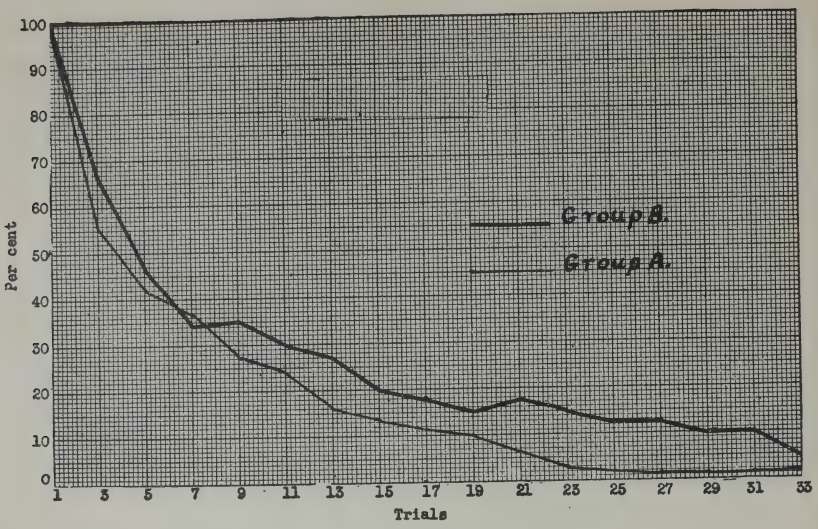


FIGURE 1*b*

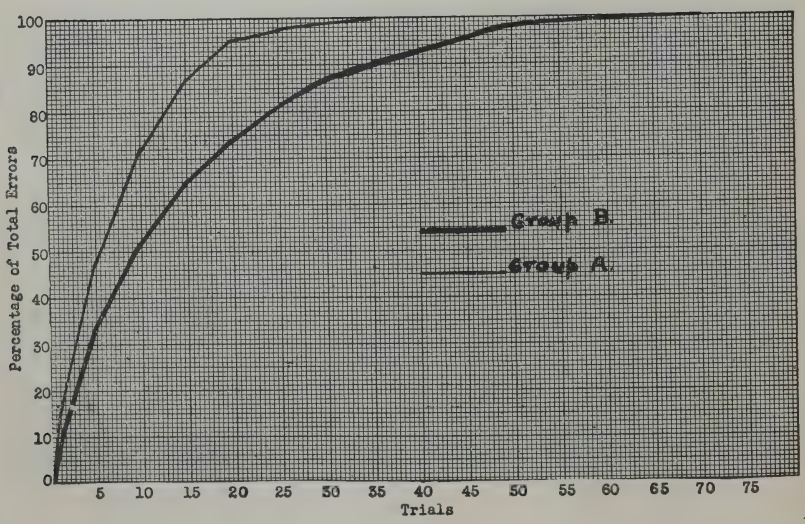


FIGURE 1*c*

structing the curve we have averaged all results except for the first trial by 2, in order to eliminate extremes of variability. Several facts are of note: (1) Both groups exhibit a rapid initial descent, i.e., a rapid initial elimination of errors. (2) At the end of the seventh trial both groups were about even in the percentage of errors still to be eliminated. (3) The most interesting, perhaps, is that, from this trial on (seventh trial), Group A eliminated errors much faster than Group B, that is, after the seventh trial Group B slowed up in their rate of learning more than Group A did. (4) The Group-A curve does not contain the steeples that are present in Group-B's curve. The curve is shown only to the end of the 40th trial.

Figure 1*c*, like 1*b*, also gives a comparison of the rate of elimination of errors. It shows the percentage of the total errors that had been made at the end of successive 5 trials.

Figure 2 shows the average time per trial record of the 2 groups. Here again we see the superiority of Group-A, though there is some crossing of the curves. Only the first 25 trials are given, as beyond that individual records affect the curves too greatly.

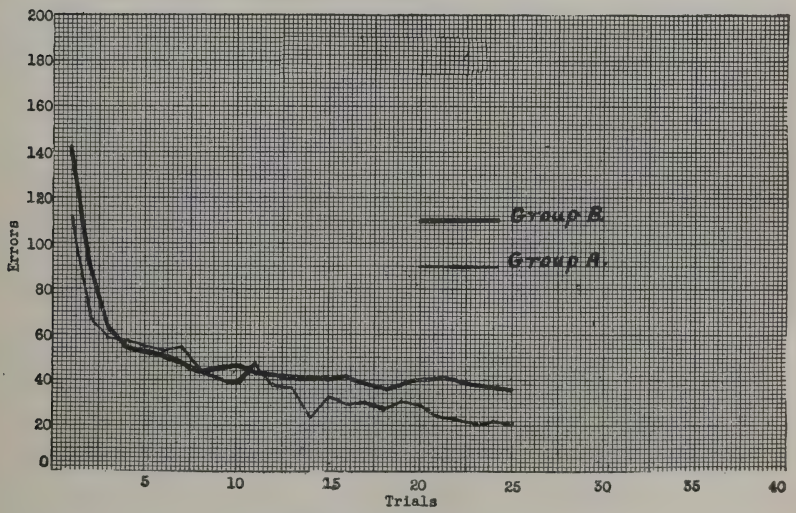


FIGURE 2

Conclusions

Any conclusions based upon the small number of subjects here experimented with are of course questionable; and we fully realize the liability of error in making such conclusions. We hope in the coming term to make a more complete study of the problem using differently patterned mazes and a greater number of subjects. Yet from the data we have already obtained it seems that we can indicate at least certain definite conclusions.

1) A large and highly significant difference was found between the 2 groups in their maze records; the group which scored high on the Thurstone test making much the better record on the maze. We may conclude, then, that the factors that make for a high intelligence score also make for a high performance on the maze.

2) The maze is highly reliable in determining group differences, but it is not so for making distinctions in individual ability.

3) Correlations indicate a fairly high positive relationship between general intelligence and motor learning as measured by learning a maze. The correlations are not highly reliable, however.

4) An analysis of the learning curves show that the learning process in the 2 groups is essentially the same, the difference being in the increased rate and greater effectiveness of the learning of the Group-A individuals.

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UNE ÉTUDE COMPARATIVE DE GROUPES D'INTELLIGENCE SUPÉRIEURE ET D'INTELLIGENCE INFÉRIEURE DANS L'APPRENTISSAGE D'UN LABYRINTHE

(Résumé)

On a fait cette étude dans le but de déterminer s'il y a un rapport entre l'intelligence générale et l'habileté dans l'apprentissage moteur comme indiquée par les résultats d'apprendre un labyrinthe. Un labyrinthe à doigt de haut relief, construit de la manière décrite par Miles (J. Gen. Psych. 1928, I) a été employé. On a mesuré l'intelligence générale par l'Examen Psychologique Thurstone, édition 1923-24. Deux groupes, chacun composé de dix étudiants, ont appris le labyrinthe. Un groupe s'est composé d'individus qui ont obtenu des résultats élevés dans le test Thurstone et l'autre groupe d'individus qui ont obtenu des résultats non élevés. Ensuite on a fait une comparaison des résultats des deux groupes dans le labyrinthe.

On a trouvé une grande différence signifiante dans les résultats des deux groupes; le groupe ayant des résultats élevés dans le test Thurstone ayant aussi les meilleurs résultats dans le labyrinthe. On a calculé aussi les corrélations entre les résultats dans le test Thurstone et le temps et les erreurs et le nombre d'épreuves nécessaires pour apprendre le labyrinthe. Ces corrélations ont indiqué un rapport positif assez élevé. Chez un groupe choisi de cette manière, pourtant, elles ne sont pas très sûres.

Les coéfficients de constance basés sur les épreuves paires-impaires dans le labyrinthe ont été élévés, montrant que le hasard n'est pas un facteur important.

On conclut expérimentalement de cette étude non complète:

I. Les facteurs qui donnent des résultats élevés dans un test d'intelligence donnent le même résultat dans le labyrinthe.

II. Le labyrinthe est très exact, du moins pour la détermination des différences de groupes.

Spence et Townsend

EIN VERGLEICH VON GRUPPEN HOHER UND NIEDRIGER INTELLIGENZ IM ERLERNEN EINES LABYRINTHS

(Referat)

Der Zweck dieser Studie war zu bestimmen ob irgend eine Beziehung zwischen allgemeiner Intelligenz und der Fähigkeit im motorischen Erlernen bestehe, nach den Leistungswerten beim Erlernen eines Labyrinths zu urteilen. Ein Labyrinth mit erhabenem Muster (high relief finger maze) nach der Beschreibung von Miles hergestellt, (*J. Gen. Psychol.*, 1928, **1**) wurde

angewandt. Allgemeine Intelligenz wurde mit Thurstones Psychologischem Examen bemessen, 1923-24 Ausgabe. Zwei Gruppen von je zehn Studenten erlernten das Labyrinth. Eine Gruppe bestand aus Studenten, die nach Thurstones Test hoch standen, die andere aus Studenten die niedrige Leistungswerte erhalten hatten. Sodann verglich man die Leistungswerte der beiden Gruppen beim Erlernen des Labyrinths.

Man fand einen grossen höchst bedeutsamen Untershied zwischen den Leistungswerten der beiden Gruppen; die Gruppe die nach Thurstones Test hoch stand, erzielte auch höhere Leistungswerte mit dem Labyrinth. Man berechnete auch die Korrelationen zwischen Thurstones Leistungswerten und Zeit und Fehler, sowie die Anzahl der Versuche das Labyrinth zu erlernen. Diese Korrelationen deuten auf eine ziemlich hohe positive Beziehung hin. Bei einer so ausgewälten Gruppe judoch sind sie nicht sehr zuverlässig.

Koefizienten der Zuverlässigkeit, auf gerade und ungerade Versuche des Labyrinths basirt, waren hoch, ein Beweis, dass der Zufall kein bedeutender Faktor ist.

Von dieser noch unvollendeten Studie zog man die folgenden vorläufigen Schlüsse: (1) Die Faktoren die hohe Intelligenz Leistungwerte erzielen, erwirken gleichfalls hohen Erfolg beim Erlernen des Labyrinths: und (2) Das Labyrinth ist höcht zuverlässig, wenigstens beim Bestimmen von Gruppenunterschieden.

SPENCE UND TOWNSEND

THE EXPERIMENTAL DERIVATION OF A NEW FORMULA FOR MENTAL WORK*

From Wells College

C. Oliver Weber

Present Dilemma of Psycho-Dynamics

Physics, declared Titchener, is the "mother" of the sciences. However, the discrepancy between "mother" and "child" is in this case more marked as regards achievement than as regards age. Both physics and psychology have a branch of dynamics, but what a vast difference in their respective attainments in this field! Transformations of physical energy are governed by fairly simple laws long since known to the physicists. In psychology, on the contrary, the subject of dynamics is like Sir Ray Lankester's "black hat, lost in a dark room." Add, as regards psychology, that it is not known whether the black hat exists or not, and the picture becomes complete. For, it is seriously questioned whether mental energy, as distinct from physical energy, exists at all.

There is one form of experience which does suggest the existence of a non-physical form of energy, namely, the experience of effort or strain. Now, the capacity of a physical mass to do work derives from its *inertia*; and *inertia* and *effort* have opposite meanings. To be inert is to follow the path of least resistance, but to experience effort is always to move against some resistance. The inert body moves only when impelled from without—and ceases to move from the same cause. In fact, physical masses *as such* are as much "at rest" while moving as they are while stationary. Curiously, so far as physical masses are concerned, the distinction between the "dynamic" and the "passive" does not exist. This distinction clearly exists introspectively between the states of effort and repose. It is in vain that we look to physical nature for the source of this distinction —it is rather the physicist who must look to psychology for an account of the fact that we regard potential and kinetic states as different in *kind*. It is not at all improbable, if the experience of

*Received for publication by H. L. Hollingworth of the Editorial Board, October 17, 1928.

effort is what it represents itself to be, that there is a separate form of energy which we may regard as *mental*. At any rate, it is only on this supposition that the subject of psycho-dynamics can have meaning.

This suggestion will be received by the average behaviorist, dominated by his "mother ideal," the worship of physics, as a case of "dabbling in metaphysics." Of course, the real puzzle is to know who the metaphysicians are. To assert, without testing the matter, that physical energy is the only form of energy that exists is itself a threadbare ontology, having the most ancient metaphysical lineage. Not knowing this, a group of modern psychologists sponsor it for its supposed empiricism. Thus, Kempf (7) finds the origin of psychical energy in muscular tensions. R. D. Gillespie (5) well exclaims that it remains a mystery how a muscular tension (which is known to use up energy) can give rise to a fund of energy. Physiologists, who presumably know the most regarding the question, "Is physical energy the sole energy exhibited by organisms?" are not so sure regarding the proper answer to it. E. D. Adrian (1), for instance, declares that mental energy can only be analogous to physical energy, not identical with it. A man and a physical mass both possess the capacity to do work; the mass does work, thanks to its inertia; but the man's work is called "mental" because it is a biological and purposive activity.

However, there is a difficulty about establishing a separate science of mental energy, quite apart from the question of its separate existence. The energy of "strain" is only accessible to introspection, and, now, *method* seems to fail us. Lehmann (see 11) found that feelings of strain and effort are sometimes useful, but too often give inconsistent and useless results in the laboratory. The most convincing of recent opinions on this point is that of Raymond Dodge. He declares that the study of the dynamic problems of mental life requires the aid of some non-introspective technique. The notions of "effort" and "strain" have not proved fruitful—"they are blind alleys in the maze of experience" (2). Evidently willing to acknowledge that the experience of strain is somehow the true "mental" energy, Dodge urges, that in investigating it, we must employ the exact methods of the physiologists. On the assumption that every psychical process is paralleled by physical changes, we may be sure that the latter can serve as a measure of the former (2, p. 7).

"The common measure of mental work is number and time. . . . . This

output conception of mental work is convenient; practically significant, since it may deal with the actual processes of business and school; and it presents plausible physical analogies. It is the yard-stick of mental achievement. Five sums in arithmetic are equal to ten pages of Dickens or half a page of German prose. We do practically measure off mental work in that way" (2, p. 7).

In all this, Dodge is careful to avoid saying that the output *is* the mental work—output is only the *measure* of mental work. What is the work measured? The work measured, says Dodge, is a mental fact to which introspection testifies, but not with the quantitative precision that a would-be science requires.

"In mechanics work may be said to be done when force moves mass against resistance. Obviously, the mechanical concept of work has no immediate application to mental life. It has neither mass, nor acceleration. Even if at some future time a mechanical interpretation of mental life might conceivably become a working hypothesis, at the present time such an hypothesis would be utterly barren and misleading" (2, p. 6).

Yet, the physiological conception of mental work must be our working hypothesis when we attempt measurement. Dodge assumes that the physiological states, though not identical with the mental states, are at least the pre-conditions of mental activity. We can determine the correlations of mental work, and "actual science knows no other extension of knowledge except correlations" (2, p. 2). It is a pleasure to note that Dodge thus rejects a certain attitude of "make-believe" current in American psychology. This attitude is expressed in the policy: "If we cannot envisage the mental realities, let us pretend that the physiological accompaniment is the mental fact."

Dodge finally accepts a telephone-galvanometer-sphygmograph, which very delicately measures pulse rates, as the measure of mental work. After very able discussion of data in this field, including his own results, Dodge concludes that "Within the limitations of my experimental data, I can say that, provided one starts from a pre-experimental condition of relatively complete physical and mental relaxation, every mental and physical activity is correlated with an accelerated pulse" (2, p. 25). After all, then, it appears that introspection must decide whether or not the accelerated pulse accompanies mental or physical work.

Urban (11) justly praises Dodge's clever methods of studying the energy changes that accompany mental work. In the writer's view, Urban also points out the reason why this careful project constitutes defeat for psychology. Urban writes:

"Let us suppose, however, that all the experimental difficulties have been overcome successfully, and that we know the energy transformations corresponding to every mental process. Does that really give us a measure of mental work? The principle of the conservation of energy compels us to refer these energy transformations to the concomitant physiological processes, in which energy can be neither gained nor lost. And no energy remains to be referred to the corresponding psychical processes. We have a complete understanding of the energy transformations which are involved in the physiological processes; but we are as far from a dynamic psychology as ever, for we cannot equate mental work with physical energy, although we can correlate them" (11, p. 273).

Logically, there are 3 ways out of the supposed dilemma that we can measure—not mental work, but only its accompaniments. (1) If mental realities cannot be measured, then so much the worse for mental realities—this is the solution called behaviorism. (2) If mental realities cannot be measured, then so much the worse for measurement. This is the alternative chosen by the vitalistic psychology of the Bergsonian sort. (3) A third way out, however, consists in directly denying the statement that direct quantitative measurement of mental processes is impossible. Such measurements were made by Fechner and his followers, and have been made ever since. The writer accepts this third alternative, and agrees with Urban (11, p. 274) that psychophysical method offers the most feasible approach to the problems of psycho-dynamics.

Certainly, if effort is a clear mental experience, then efforts can be compared and equated; and if its manisfestations are governed by law, we achieve a science of mental dynamics when we discover such laws. We may claim for this science the virtue of practical usefulness, lauded by Dodge. For, if "five sums in arithmetic are equal to ten pages of Dickens," is it not because they represent *equal efforts?* What else about this equation is "equal"? If we had only mathematics to guide us, we should require a page of one as equal to a page of the other. It is the inner experience of labor which leads us to understand why we require less output in arithmetic. Otherwise, we should regard the ability of students to "do" 2 pages of Dickens for every sum in arithmetic as a fact, but a miraculous one. But the critics will say that introspective differences are chronically "vague," and that the difference between the difficulty of Latin and French is subjectively indefinite. Well, if the difference is vague and indefinite, it is not practical to measure it. If the difficulty of Latin is not "just perceptibly different" from the difficulty of French, then, so far as mental work is a practical problem, an hour of one is equal to an hour of the other. It is so easy for psychologists to

forget that the "exactness" with which we read a thermometer is solely due to the grossness of our units of measurement. That is, all observers agree about the reading of the thermometer (or any other objective instrument) because the scale divisions are far above the threshold of sense perception. As our scale divisions become exact, i.e., as they become minute, all judgments become psychophysical judgments.

The writer has selected the field of voluntary muscular movement as the ground for an attempted psycho-dynamics of a truly psychological sort. Conscious and effortful muscular movement is surely one of the simplest forms in which mental work expresses itself. We have here the 3 variables which enter into the work formulae of physics, namely, the mass moved (M), the time consumed (T), and the distance traversed (D). In physics, the capacity of a moving mass to do work is given by the formula $1/2\ M \cdot V^2$. Are distance, time, and mass related to each other in this peculiar way when we are consciously equating muscular efforts? The outcome of the following experiments indicates that *the formula involved in mental work is probably different from the physical formula given above*: that, in mental work, the relation between these 3 variables is given by the formula $1/2\ M \cdot \sqrt{V}$.

Experimental Procedure and Results

We employed 4 subjects in an extended experiment in which subjectively equal muscular efforts were produced by horizontal movements of the forearm. The basic piece of apparatus employed was the kinaesthesiometer devised by Michotte, and described by the present author in a previous study (13).

Briefly, the apparatus was as follows: the kinaesthesiometer consists of a horizontal metal beam, which carries the forearm of the subject. This beam is housed at one end in a ballbearing socket, while the other end moves freely along a curved, graduated scale. A heavy metal frame, provided with screws for leveling, forms the base of the instrument. To the curved scale, a sliding carriage was added, which can be loaded at will. The O's push the carriage along the scale by means of the moving beam. The resistance of the carriage could be varied by use of 1 of 3 lead weights.

The resistance involved is given by the well-known law, that the friction between 2 constant surfaces is directly proportional to the forces pressing them together. However, the 3 resistances which figure in our experiments were not taken as the simple weight of

the lead pieces. We determined experimentally how many grams of weight would move the carriage loaded respectively with the 3 weights, and thus get the 3 loads: 140, 176, and 235 grams. This gives us the 3 "masses" involved, and since the curved scale gives the distances to the accuracy of 1 mm., we require only the time of the movement to complete the 3 quantities.

A Dunlap chronoscope, run from a rectifier, was so attached that the time of each movement was automatically measured. So much for apparatus.

A serious defect in previous arm-movement studies made by some psychologists consisted in employing instruments (like the ergograph) (6) in which the force of the movement is automatically correlated with the *extent* of the movement. This prevents distance and effort from being *independent variables.* The work of J. J. B. Morgan (10) suffers from a less serious defect, that of having one of the variables (in his case, distance) always constant. His 8 *O*'s reproduced forces, extents, and time intervals by pulling on a handle connected with sets of springs, which in all cases had to be pulled just 175 mm. No wonder, then, that he gets, as one of his major results, the fact that distance, the *extent* of the movement, is the most important criterion in reproducing force.

As regards procedure, we were fortunate in having the previous researches of Fullerton and Cattell (4), Leuba (9), Kostin (8), and Hollingworth (6) for our guidance. Our previous study of space and time perception confirms the belief of both Cattell and Hollingworth that the best method is the method of average error, in which the observer has the task of making a comparison movement as much as possible like a standard movement.

We also permitted the *O*'s to set their own standards, in order to avoid the "illusion of impact" which results when the standard movement is limited by an obstruction against which the carriage strikes at the terminus of a movement. The reality of this illusion is amply demonstrated by Hollingworth. When the *O*'s set their own standards, then both the *S* and the *C* movements are active, thus avoiding the possible systematic errors which may attend the active reproduction of a passively impressed movement.

We did not vary the initial starting position of the movements, although Leuba took the precaution to do this in his experiments. His purpose was to eliminate the sense of initial position as a criterion in guiding the reproductions. Our study is not a search for the

criteria employed by *O* to reproduce work, but to determine the relations between the mass, distance, and time of movement, whatever the criteria may be.

For the same reason, we did not attempt to keep constant the time interval between the standard and the reproducing movement. In truth, previous investigators find that the time interval between the *S* and *C* movements has little effect on the accuracy of the reproduction, so long as this interval is fairly short. Fullerton and Cattell [see Hollingworth, (6, p. 63)] find that the approximate probable error does not seem to increase as long as the interim does not exceed 9 seconds. Th. Schneider (6, p. 63), studying curvilinear movements on the hand rotating on the wrist joint, finds that the error remains constant up to 2 minutes. Münsterberg, Landau, Hollingworth, and others report similar findings. Our own interval between the *S* and the *C* movement was about 3 seconds.

It must be noted that all *O*'s gave attention only to the movements made away from the body, with the right arm. After such a movement, the beam was immediately returned to the starting position, leaving the loaded carriage to mark the distance of the excursion. The experiments fall into 2 portions. In the first experiment, the *S* and *C* movements were always unequally loaded. Since there were 3 loads in all, this gives us 6 sorts of paired movements. There was a preliminary period of practice, consisting of 30 reproductions for each *O*, but without knowledge of success. The object of the practice period was to familiarize *O* with the task of concentrating, not on objective distances and times, but upon her inner experiences of effort and strain. The *O*'s were made to understand that they were to make the *C* movement equal to the *S* movement in its subjective *effort*, and that external criteria were to be ignored.

After the practice series, further determinations were made until we had gathered, for each *O*, 20 reproductions for each one of the 6 possible pairs of movements. Counting both *S* and *C* movements, this gives 240 movements for each subject, or 960 in all.

In a second series, we had the *O*'s make paired movements where *S* and *C* were equally loaded. Since there were 3 loads used, this gives 3 additional pairs of movements. Counting the separate movements, we get 480 movements in all from the 4 subjects, giving a total of 1440 (when added to the 960 of Part 1) movements, where each movement yields a mass in grams, a distance in centimeters, and a time interval in terms of *sigma*.

In the further handling of these raw data, our problem became as follows: What formula, employing M, T, and D as variables, best expresses the equivalence of 2 efforts? To be sure, we here employ the same identical measures of M, T, and D which enter into the formulae of physical work, but this does not presuppose that the resulting formulae must be a physical one. The formulae, $1/2\,M \cdot V^2$ is a physical formula, *not because of any sign inherent in itself,* but because it actually fits the facts regarding the energy required to raise physical motions to certain momenta; or, conversely, the energy which masses having a certain velocity can yield. In the same way, the formula $1/2\,M \cdot \sqrt{V}$ becomes a psychological formula when it is confirmed that mass and velocity always combine in this peculiar fashion when effort is reproduced consciously.

It is necessary to emphasize the consideration that the employment of the concepts of mass, time, and distance does not, *ipso facto,* indicate physical formulation. For, it is a mooted point whether M, T, and D are concepts which are of psychological or of physical origin. That is, the statement that 3 ounces are less than 3 pounds has logical meaning perhaps only because we have, prior to the meaning, *psychological* acquaintance with ounces and pounds (12). Similarly, one may argue, that the statement, "A mile contains 320" has numerical accuracy, but can have meaning only when we complete it, saying, "A mile contains 320 rods." And only the person who has walked a rod can understand what the statement means. This is to say, that the term "rod" gives meaning to the term "320," not vice versa. Rods are not numbers, but, first of all, *experienced intensities.* Indeed, one might defend the Kantian position in general, and declare that the notions of time and distances (and masses also) are *psycho-genetic,* so that any formula which employs them is anthropomorphic. This would be the converse of the usual error, which is to suppose that any formula, into which M and T and D enter as variables, is perforce a physical formula. Whatever the origin of the meaning of space and time (and the present conflict in physical science involves precisely the question of their origin), we shall assume, in this study, that, if mass, time and distance unite so as to give a unique formula when mental work is done, *that is what we mean by declaring that mental work is a species distinct from physical work.*

We, therefore, seek to determine, by a process of "fitting," the formula which gives the least average error when applied to the

paired *S* and *C* movements. After preliminary trials, we selected 3 formulae for application to the data, in an attempt to determine which one makes the best fit. To put the problem in question form, we are asking, "When *O* reproduces the standard movement, what does she reproduce: (1) the product of the mass times the distance ($M \cdot D$), (2) $1/2\,M \cdot V^2$, or (3) $1/2\,M \cdot \sqrt{V}$?" Hereafter, we shall generally refer to these formulae by number, as just indicated. If these formulae are applied to the data of each standard movement, and then to the corresponding comparison movement, which one secures the least variability of *C* from *S*? We will assume that the one giving the least error is the correct formula; on the assumption that if a formula actually gives a small Av. E. that this could not have been due to accident any more than the data which fit any formula could be an accident: it could only be the expression of the *order* of phenomena.

We reserved for this point a certain feature about our apparatus which takes on especial significance here. The carriage of the kinaesthesiometer offers 2 sorts of resistance to any force acting upon it. There is, first, its property of *inertia;* and because of it, to increase the velocity of the carriage arithmetically, the energy expended must increase as the square of the velocity. On the other hand, there is the resistance due to the sliding friction which is practically independent of the velocity. For all ordinary velocities (within which our velocities fall) the resistance due to friction remains constant; hence it "cancels out," leaving the remaining resistance constantly governed by the formula, $1/2\,M \cdot V^2$.

We give, in Table 1, the main results of the first set of experiments where *S* and *C* are never equally loaded. The 6 horizontal columns of figures represent (as the captions indicate) the 6 possible combinations of the *S* and *C* movements. In these columns, opposite the names of the observers to the left, are written the average percentages of the deviation of the *C* from their corresponding *S* movements, when computed according to formulae Nos. 1, 2, and 3, respectively. The deviations of *C* from *S* were taken regardless of sign, i.e., regardless of whether they were a plus or a minus deviation from *S*.

In order to save space in this and the following tables, we do not record the raw data from which we get the percentages of deviation. Suffice it to say that these exhibit the fact that much of the error is *systematic* error, due to the tendency to reproduce distances.

TABLE 1

AVERAGE PERCENTAGES OF VARIATION OF *C* FROM *S*, WHEN *S* AND *C* ARE UNEQUALLY LOADED

O	Formulae	*S* 140 *C* 176	*S* 176 *C* 140	*S* 140 *C* 235	*S* 235 *C* 140	*S* 176 *C* 235	*S* 235 *C* 176
Mai.	$M \cdot D$	.148	.200	.258	.217	.150	.258
	$1/2M \cdot \sqrt{V}$	.116	.188	.308	.322	.075	.183
	$1/2M \cdot V^2$	.332	.494	.437	.346	.520	.452
Gr.	$M \cdot D$	.178	.187	.171	.098	.235	.117
	$1/2M \cdot \sqrt{V}$	.126	.204	.258	.232	.213	.143
	$1/2M \cdot V^2$	.450	.400	.472	.670	.485	.594
Mac.	$M \cdot D$	.110	.099	.122	.117	.153	.085
	$1/2M \cdot \sqrt{V}$	.108	.161	.292	.243	.151	.137
	$1/2M \cdot V^2$	.351	.345	.424	.581	.372	.469
Mor.	$M \cdot D$	.081	.073	.225	.119	.227	.202
	$1/2M \cdot \sqrt{V}$	.105	.135	.145	.204	.106	.145
	$1/2M \cdot V^2$	.549	.287	.634	.764	.609	1.330
Averages:							
	$M \cdot D$	.129	.140	.196	.138	.191	.165
	$1/2M \cdot \sqrt{V}$	.114	.172	.250	.250	.136	.152
	$1/2M \cdot V^2$	.420	.381	.493	.590	.494	.661

TABLE 2

AVERAGE PERCENTAGES OF VARIATION OF *C* FROM *S*, WHEN *S* AND *C* ARE EQUALLY WEIGHTED

O	Formulae	*S* 140 *C* 140	*S* 176 *C* 176	*S* 235 *C* 235
Mai.	$M \cdot D$	.085	.057	.085
	$1/2M \cdot \sqrt{V}$	.072	.053	.055
	$1/2M \cdot V^2$	.275	.213	.180
Gr.	$M \cdot D$	.086	.086	.099
	$1/2M \cdot \sqrt{V}$	.106	.086	.095
	$1/2M \cdot V^2$	.381	.334	.331
Mac.	$M \cdot D$	.052	.077	.088
	$1/2M \cdot \sqrt{V}$	.076	.062	.059
	$1/2M \cdot V^2$	.274	.225	.283
Mor.	$M \cdot D$	.111	.121	.140
	$1/2M \cdot \sqrt{V}$	.082	.126	.117
	$1/2M \cdot V^2$	.376	.533	.495

For instance, after setting a standard effort with the 140 gram weight, the observers tend to reproduce too much distance in reproducing this standard effort with a heavier weight. The converse error is made when a heavy standard is followed by a lighter comparison movement. The lowest line of figures gives the average percentages of error, regardless of subjects.

In Table 2 we present the corresponding data for the second part of the experiment, where S and C are always equally loaded.

In Table 2 the notable feature is the general decrease in the size of the deviations of C from S, as compared with Table 1. This is no doubt due to the fact that since S and C are equally loaded, the tendency to reproduce distances now has the effect of decreasing the error, whereas formerly it tended to increase it.

The question of central interest, however, is not the matter of absolute percentages of error, but the comparison of the errors yielded by the 3 formulae. In Table 3 we give a synopsis of results, based on the 2 preceding tables. It gives a final average of all percentages of error belonging to each observer.

TABLE 3

AVERAGE PERCENTAGES OF ERROR FOR EACH OBSERVER

O	$M \cdot D$	$1/2M \cdot \sqrt{V}$	$1/2M \cdot V^2$
Mai.	.140	.129	.361
Gr.	.127	.145	.457
Mac.	.093	.123	.369
Mor.	.139	.116	.619
Averages	.125	.128	.452

It is at once clear that the third (physical) formula is the "worst" of the 3: it gives an error almost 4 times as great as the error yielded by the second formula. The truly "rival" formulae are the first and the second: the first one is the best for 2 of the subjects (Gr. and Mac.) and the second one is best for the 2 remaining subjects (Mor. and Mai.). The averages for all subjects show that the first 2 formulae apply about equally well, the first formula showing an advantage of only .003%. This peculiar parallelism between these 2 formulae seems to obtain throughout.

A further computation was made to determine how the first 2 formulae "come out" when applied to the first fourth and the last fourth of the data. We anticipated that the first formula, since it

does not embrace the time variable and lays so much stress on distance, would show less variability for the last one-fourth of the data, when the processes have become automatized. Table 4, however, shows no such result.

TABLE 4

ERRORS YIELDED BY FORMULAE 1 AND 2, WHEN APPLIED TO FIRST AND LAST FOURTHS OF DATA

	First fourth of data		Last fourth of data	
O	$M \cdot D$	$1/2M \cdot \sqrt{V}$	$M \cdot D$	$1/2M \cdot \sqrt{V}$
Mai.	.136	.138	.171	.176
Gr.	.146	.176	.148	.170
Mac.	.108	.151	.096	.140
Mor.	.118	.125	.156	.145

Curiously enough, in the cases of 2 subjects (Mai. and Mor.), both formulae show larger errors for the last fourth than for the first fourth of the data, and this holds true for both formulae. Since all *O*'s worked without knowledge, much improvement due to practice could hardly be expected. The separate sittings were long (1 hour) for each *O*; hence the last one-fourth of the data, coming at the end of a period, owes its large error to fatigue. Incidentally, the 2 subjects showing the great increase in error in the last fourth of the data were involved in a "Junior week-end." All subjects, especially untrained ones, find a routine of prosaic arm movements difficult to attend to. The author has grounds for supposing that the second or "psychological" formula will apply just to the degree that *O*'s movements are intensely voluntary and conscious.

The most constant finding contained in Table 4 is the likeness between the 2 formulae when they are applied to the same data, (whether first or last). This resemblance may be due to the fact that, although time does not enter into one of them, distance enters into both of them in the same way, i.e., so as to cause the error to take the same direction. We are thus obliged to say that, although time enters into the second formula only, it enters into it in such a way that both formulae are equally satisfied. This suggests, as Descartes discerned, that physical and mental formulae need not be mutually exclusive.

The data so far presented are not a final test of the question, "Is Formula 1 or is Formula 2 the more applicable to the data?" This is because we have so far considered the average variation only, and

not the constant variation. It may well be that if we consider the constant error, whether constantly plus or minus, one formula may fit better than the other. Physically, the *S* and the *C* movements were made under almost identical conditions; and a coiled spring, moving these various weights, would yield no constant error at all. But a conscious subject, whose present action is beset by the residuum of recent actions, becomes the victim of systematic errors with which the annals of psycho-physics are replete. The more introspective the procedure, the more clearly will these systematic errors be exhibited. The ordinary illusions of vision are marked, just to the degree that the observer forgets standard physical measurement and pays attention to the psychological impression alone. This should hold true in the present experiment. Our *O*'s were equating "efforts," but there is a tendency to reproduce distances. But to combat this tendency also costs effort; hence, to pay attention to effort is to give full play to the tendency to reproduce distance. The object of psychology, of course, is not first to eliminate systematic errors, but to detect them. Although the tendency to reproduce distance introduces systematic error, it may not introduce any greater deviation from a constant. In order to supplement the data of our first 4 tables, we prepared correlations between the standard and comparison movements when treated by the first and then by the second formula. The assumption is that, if the first formula actually fits the paired *S* and *C* movements better than does the second formula, then the *r* between the *S* and *C* movements should be higher when the first formula is applied to them. We prepared a separate correlation (and *P.E.*) for each one of the possible combinations of *S* and *C*. The data of all 4 *O*'s entered into the *r* between the *S* and *C* movements of the same kind, thus giving 80 cases upon which each *r* is based. We present the resulting *r*'s and *P.E.*'s in Table 5.

It will be seen that, when *S* and *C* are equally loaded (as indicated by the headings of the vertical columns), the *r*'s between the *S* and *C* movements are much higher than in the cases (the first 6 columns) where *S* and *C* are unequally loaded. But the *r*'s given by both formulae, when applied to the same data, are remarkably alike, when a general average is considered. The average *r* for the first formula is +.518, while for the second formula it is +.528. The second formula shows a slight superiority, whereas the first 2 tables of our study show a slight superiority for the first formula. However, when we consider the *r*'s yielded when *S* and *C* are equally loaded, the

TABLE 5

CORRELATIONS BETWEEN PAIRED *S* AND *C* MOVEMENTS

Formula	*S* 140 *C* 176	*S* 176 *C* 140	*S* 140 *C* 235	*S* 235 *C* 140	*S* 176 *C* 235	*S* 235 *C* 176	*S* 140 *C* 140	*S* 176 *C* 176	*S* 235 *C* 235
$M \cdot D$	$r=+.642$ $P.E.=\pm.043$	$+.348$ $\pm.066$	$+.543$ $\pm.052$	$+.650$ $\pm.043$	$+.422$ $\pm.062$	$+.506$ $\pm.055$	$+.873$ $\pm.018$	$+.878$ $\pm.017$	$+.882$ $\pm.017$
$1/2M \cdot \sqrt{V}$	$r=+.518$ $P.E.=\pm.055$	$+.725$ $\pm.035$	$+.459$ $\pm.059$	$+.593$ $\pm.049$	$+.557$ $\pm.052$	$+.318$ $\pm.067$	$+.383$ $\pm.064$	$+.621$ $\pm.046$	$+.832$ $\pm.023$

great superiority of the second formula is at once evident. All 3 of these r's are +.8 or more. Again we may ascribe this high correspondence between S and C to the fact that the tendency to reproduce distance becomes a powerful aid to the O's. It will be noted that, although these 3 r's are high, they do not vary with the weight used. On the other hand, the r's between equally weighted S and C movements, when treated by the second formula, *increase markedly as the load increases from 140 to 235 gram.* This is also evident in Table 2, which shows that, as the weight increases (and S and C are equally weighted), the average variation of C from S decreases slightly for the second formula. For the first formula, the average variation actually increases.

General Results and Conclusions

Both the average errors (treated in Tables 1-4, inclusive) and the correlations (Table 5) show that the physical formula which governs the relation between energy and output in the physical world does not apply to the work of a living subject, though the variables of mass, distance, and time enter into both situations. However, there are 2 other formulae ($M \cdot D$ and $1/2\,M \cdot \sqrt{V}$) which fit the case of conscious muscular work equally well.

Nevertheless, there are strong grounds for supposing that the second formula ($1/2\,M \cdot \sqrt{V}$) is peculiarly a "psychological" formula. The grounds for this contention are as follows:

1) That the formula $M \cdot D$ applies so well is due to the capacity of the subject to "learn." Through long experience, we measure the amount of work by the distance through which load must be carried. Distance thus becomes the vicarious sign of the amount of the work done. Hollingworth regards this general fact as his most important conclusion regarding the disputed question of the *criteria* by means of which O's reproduce distances, times, and efforts.

"Movements are judged to be equal which have been *learned to be equal.* Innumerable secondary and essentially unrelated criteria may be utilized in the recognition and in the judgment" (6, p. 81). In our own results, when distance *can* serve as the measure of work, it becomes the "unrelated criterion," which replaces the subjective experience of work.

2) But, *as heavier weights are used, the subjective experience of effort is intensified,* and the second or "psychological" formula becomes more applicable.

3) Thirdly, the second formula is the one which concerns psychology for the reason that it embodies the time variable; and it is a general belief that time plays a fundamentally different rôle in mental than in physical phenomena. In the last analysis it is doubtful whether time affects physical phenomena in their *quantitative* aspect at all. Is not a machine a machine, because, no matter how long it endures, it is always the same? At the root of the inertia of matter lies its incapacity to learn. Consider a coiled spring, or a lifted weight, containing potential energy. The amount of this energy which may become kinetic does depend on the rate at which the conversion takes place, whether a minute or a day. To produce high kinetic velocity, a spring must liberate energy in amounts equal to the square roots of these velocities. Yes, but these resulting velocities of the mass upon which the spring acts only accumulates the exact amount of energy put into it, which it will return in full upon impact. But, for this guaranteed conservation of energy, the physical world sacrifices the capacity to learn. At the end, the universe is quantitatively the same—it is as though time did not exist.

4) But, to turn to the specific difference between the psychological and the physical formula, we call attention to the following facts: The main fact expressed by the physical formula $1/2\,M \cdot V^2$ is that *high velocities are not economical velocities,* at least not economical of energy. Now, numerous facts of psychology suggest that when conscious motor, sensory or intellectual work is done, the quicker processes are more economical (proportionally so) than slower ones. The second formula embodies this condition. If this formula is true to the facts, then, when mental output increases arithmetically, the energy output increases, but in *decreasing* proportion.

The evidence that this is actually the case is suggested by the results of numerous investigations in the field of movements and in other fields. These data are far too rich and significant to be cited in full in support of our present results. They deserve a separate and extended summary. As samples, we may consider first the well-known fact of size-weight illusion. Of 2 boxes of the same weight, but of unequal size, the larger one "seems" lighter. We except the classic explanation with one alteration, which removes its one weak point. This illusion is said to be due to defeated expectation—we expect the larger box to be heavier, "innervate" for a heavier weight, and then, due to the effect of contrast, we *judge* it to be lighter than it really is. We italicize the word "judge" because it is the weak

link in the argument. How could an error of judgment, conscious and logical, make the larger box actually *feel* lighter? In Helmholtz' explanation of visual contrast, psychologists have long since found a similar weakness. According to our view, the larger box *is* psychologically lighter. To say that it "seems" to be lighter is an after-thought, due to our faith in a pair of balances. The larger box *is* lighter, judged by the effort required to lift it, because it is lifted more quickly. To the balances, no doubt, both boxes are of the same weight, but let psychology stick to the psychological realities. Flournoy (22) shows that the larger a body the quicker it is raised. The excellent summary of facts given in chapter 14 and 16 of Myers' *Text-Book of Experimental Psychology* supports our contention that time enters into the economy of mental work in a unique way. A dozen other researches are in harmony with our results—those of Dearborn, Münsterberg, Woodworth, Kostin, and others.

To be sure, research study of the learning process (Ogden) has shown that there is a more efficient rate, which varies with individuals. But an individual's rate cannot always be mechanically constant. It is the most natural to suppose that when a subject is engaged in mental tasks, an effortful *nisus* of the attention should solve the problem in less time. We think that *this voluntary direction of the attention* comes very near being the essence of *mental work*. No doubt, as the problems become more difficult, the time taken to solve them must be longer. But, for a problem of given difficulty, increase in effortful concentration may be more economical than a more attenuated effort. The less our tasks require attention, the less they are mental tasks; and, accordingly the less would higher speed be economical. On the contrary, when performances become automatized, they are taken care of by the body, not the mind; hence, high rates of effort become uneconomical.

We conclude, however, by expressing the greatest diffidence about our results, and we will especially welcome adverse criticism.

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LA DÉRIVATION EXPÉRIMENTALE D'UNE NOUVELLE FORMULE POUR LE TRAVAIL MENTAL

(Résumé)

A peu près tous les étudiants américains du sujet du travail mental et de la fatigue mentale (Psycho-dynamique) ont appuyé sur le besoin de quelque technique non introspective. Raymond Dodge donne l'exposition et la défense les plus claires de ce principe de la Méthode. Nous connaissons le travail mental le plus généralement comme l'expérience de "l'effort" et "grand effort," mais selon Dodge de tels jugements nous conduisent à des "culs-de-sac" dans le champ de l'expérimentation. Nous pouvons réussir seulement si nous prétendons que chaque processus mental a un processus physiologique parallèle; et que nous devons accepter la mesure de celui-ci comme moyen de mesurer celui-là.

Mais nous pouvons juger quantitativement nos expériences de l'effort. L'expérience suivante donne une loi tentative qui gouverne le travail mental comme il se manifeste introspectivement, et cette loi s'oppose à la formule qui gouverne le travail des machines. Quatre sujets ont fait 1440 essais de reproduire le même effort de mettre en mouvement une voiture chargée (kinesthésiomètre Michotte). Le temps (en sigma), la distance (en centimètres), et la charge (en grammes) ont été notés pour chaque mouvement. On a appliqué trois formules à ces données pour constater laquelle donnerait la moindre déviation des mouvements de comparaison de leurs mouvements étalon respectifs. La formule physique ordinaire, montrant le rapport entre les quantités de l'énergie et le temps et les distances des mouvements ($1/2$ M. V^2) appliquée aux résultats, donne la déviation moyenne de la comparaison du mouvement étalon comme de 45 pour cent. Mais la formule $1/2$ M. $\sqrt{V}$ appliquée de la même manière aux mouvements mis en paires donne une déviation de seulement 12 pour cent.

Toutes les sciences prétendent que cette formule est vraie laquelle est la

plus conforme aux données. Aussi la deuxième formule est-elle plus vraie que la formule physique. Nous prétendons que, si une formule est conforme aux données, ce n'est pas par hasard mais c'est l'expression objective de l'égalité subjective des efforts éprouvés par nos sujets.

WEBER

DIE EXPERIMENTELLE ABLEITUNG EINER NEUEN FORMEL FÜR GEISTIGE FÄHIGKEIT

(Referat)

Fast jeder amerikanische Forscher im Gebiet geistiger Tätigkeit und geistiger Ermüdung (Psycho-Dynamik) hat die Notwendigkeit einer nicht-introspectiven Technik betout. Raymond Dodge gibt die klarste Darlegung und Begründung dieses Prinzips der Methode. Geistige Tätigkeit ist uns am besten bekannt als die Erfahrung von "Bemühung" und "Anstrengung"; doch solche Urteile führen uns, sagt Dodge, nur in "Sackgassen" in dem Gebiet des Experiments. Wir können das Ziel nur erreichen wenn wir annehmen dass jeder geistige Vorgang von einem parallel laufenden physiologischen Vorgang hegleitet wird; und dass das Mass des Letzteren als Mittel zum Bemessen des Erstern diene.

Doch wir dürfen quantitative Urteile über unsere Erfahrungen im Benehmen äussern. Das vorliegende Experiment ergibt ein vorläufiges Gesetz welches geistige Tätigkeit in ihrer introspectiven Äusserung beherrscht und dieses Gesetz bezeigt einen Gegensatz zu der Formel die Maschinenarbeit beherrscht. Vier Vpn. machten 1440 Versuche dieselbe Anstrengung beim Bewegen eines beladenen Wagens zu reproduzieren (Michotte Kinaesthesiometer). Die Zeit (in Sigma), Entfernung (in Zentimetern), und die Last (in Grammen) wurden für jede Bewegung verzeichnet. Man versuchte drei Formeln mit diesen Daten um gu erfahren welche die geringste Abweichung der zu vergleichenden Bewegung von den normalen Bewegungen zeigte. Die gewohnliche physikalische Formel der Beziehung zwischen der Kraftmenge, und Zeit und Entfernungen der Bewegungen, diesen Daten angepasst ($i/2\ M.\ V^2$) ergibt eine durchschnittliche Abweichung des Vergleichs von der normalen Bewegung als .45. Doch die Formel $i/2\ M.\ \sqrt{V}$ den gepaarten Bewegungen in gleicher Weise angepasst, ergibt eine Aweichung von nur .12.

Alle Wissenschaften nehmen an dass jene Formeln "wahr" sind, die sich den Daten am besten anpassen. Daher dürfen wir behaupten dass die zweite Formel "wahrer" ist als die physikalische Formel. Man behauptet, dass wenn eine Formel den Daten überhaupt passte, so könnte dies kein Zufall sein, sondern die objective Äusserung der subjectiven Gleichheit der Anstrengungen die unsere Vpn. fühlten.

WEBER

SHORT ARTICLES AND NOTES

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Walter Miles
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THE PSYCHOGALVANIC REFLEX IN HYPNOSIS

G. H. Estabrooks

The experimental work herein described was begun at Harvard. The writer was then obliged to discontinue his observations for over a year, finally bringing them to a conclusion at Colgate University.

The psychogalvanic reflex apparatus was in the very compact form known as Waller's Emotometer put out by Gambrell Bros. of London. This consists of the usual Wheatstone's bridge with a D'Arsonval galvanometer lamp, scale and shunt. The current is passed through the hand of the subject, 2 zinc electrodes being employed, one attached to the back, the other to the palm. These are held in place by heavy elastic bands. Pieces of blotting paper, 1½ inches square, soaked in a saturated solution of common salt, are inserted beneath the electrodes to give the best possible contact.

Depth of hypnosis was judged by introspective reports and by inability to resist suggestions as described in Bernheim's "Suggestive Therapeutics." While the writer would agree with P. C. Young (6) or with W. R. Wells (5) that these stages of Bernheim do not appear spontaneously but merely as a result of suggestion, nevertheless they do form a very convenient gauge to the depth of hypnosis. Thus amnesia was always taken as signifying the deepest stage, since it can only be produced by suggestion in a limited number of easily hypnotized individuals.

While this work was at first undertaken for entirely different purposes, it soon narrowed down to an observation on the very peculiar behavior of the reflex and of the gross skin resistances in the hypnotic state and in hypnotic subjects under certain conditions outside the hypnotic state.

Hypnotism may or may not be a type of sleep, but probably sleep can be induced if we use Bernheim's method, wherein sleep is definitely suggested. Hence it is an easy guess that in hypnotism so induced the gross resistance to a direct current would be much greater than in ordinary sleep. Richter (3) finds the phenomenon in sleep and the writer always found it to be the

case in hypnosis with one exception. Likewise it would seem that the reflex should tend to vanish in hypnotism as it does with a heightened resistance in the normal state. This also turned out to be the case. In this finding the writer differs from Georgi (2), but the difference may very easily be due to his method of inducing hypnosis. Had a method such as that employed by W. R. Wells been used, results might have differed.

However, one very curious and interesting reaction was found which is the main reason for submitting this article. It developed that the resistance not only increased in the hypnotic state, which was only to be expected under the technique employed, but that it also tended to increase with the mere presence of the operator. Thus in a typical somnambulist it was found that resistance would at first tend to soar only as hypnotism was induced. But after the subject had been hypnotized a number of times the mere presence of the operator in the room would cause this phenomenon. The subject would apparently be quite wide awake all the time, would answer questions in a perfectly natural manner and insist he was in no way sleepy. Yet his gross resistance would increase in exactly the same manner as if he were actually being hypnotized.

The subjects used were always graduate students or seniors. The writer acted as operator in about half the cases but preferred to have the actual hypnotizing done by a third party when possible since he could then give his full attention to the apparatus. The length of the séances varied but this appeared to have little significance. The drying of the pads on the hand in even one hour's time did not appear to greatly affect the gross resistances or the magnitude of the reflex.

The procedure was to allow the patient to sit quietly for 10 minutes with the contacts on his hands. During this period he was given certain tests to measure the magnitude of the reflex. At the end of 10 minutes' time he was hypnotized and then, after a varying period, awakened. This period varied because other experiments were being run in some of the cases. This necessitated more time than would be necessary merely to take the observations required for the purposes of this article.

In Table 1 the readings of gross resistance which are the most significant have been given. Readings were actually recorded every minute, but such a detailed record is not necessary to illustrate the point in question. In the first column we have the number of the subject and the number of the séance. While the séances are always given in their chronological order as they apply to the various subjects, the subjects are so grouped as to illustrate the point under discussion. Thus subject No. 4 was the first individual in whom the reflex under hypnosis was tried, while No. 3 was well on towards the last in the chronological order. The order of the séances in each subject was always as given, however.

The second column gives the resistance immediately after the controls were affixed to the hands. The third is the reading just before hypnosis, or

TABLE 1

	Resistance just before hypnotism	Initial resistance	Maximum resistance in hypnotism	Resistance just before awakening	Resistance after awakening
Subject 1					
Séance No. 1	15,000	14,500	62,000	60,000	19,500
" No. 2	14,000	16,000	64,000	61,000	17,500
" No. 3	17,000	19,500	64,000	63,000	14,500
" No. 4	25,000	36,000	74,000	72,000	12,500
" No. 5	59,000	64,000	67,000	66,000	15,500
" No. 6	64,000	74,000	76,000	75,500	15,500
" No. 7	15,000	44,000	78,000	75,000	14,500
Subject 2					
Séance No. 8	12,500	11,000	69,000	58,000	14,000
" No. 9	16,500	17,000	73,000	72,000	15,000
" No. 10	14,000	17,000	64,000	64,000	14,500
" No. 11	15,000	20,000	68,000	67,000	15,000
" No. 12	30,000	37,000	63,000	63,000	15,000
" No. 13	61,000	65,000	66,000	64,500	14,500
" No. 14	84,000	80,000	80,000	75,000	15,000
" No. 15	15,500	27,000	75,000	74,000	17,500
Subject 3					
Séance No. 16	12,500	15,000	30,000	30,000	12,000
" No. 17	8,000	10,000	53,000	53,000	13,000
" No. 18	16,000	15,000	49,000	49,000	22,000
" No. 19	23,000	35,000	75,000	22,000	36,000
" No. 20	42,000	56,000	67,000	63,000	18,000
" No. 21	14,000	13,000	58,000	52,000	20,000
" No. 22	17,000	15,000	46,000	46,000	20,000
Subject 4					
Séance No. 23	68,000	71,000	71,000	70,000	15,500
" No. 24	82,000	81,000	81,000	80,500	21,000
" No. 25	74,000	75,000	75,000	74,000	17,000
Subject 5					
Séance No. 26	130,000	76,000	86,000	66,000	17,000
Subject 6					
Séance No. 27	18,000	57,000	60,000	56,000	18,000
Subject 7					
Seance No. 28	26,000	15,000	53,000	48,000	14,000
Subject 8					
Séance No. 29	22,000	17,000	22,000	17,000	12,000
Subject 9					
Séance No. 30	11,000	12,000	34,000	18,000	13,000
Subject 10					
Séance No. 31	43,000	30,000	70,000	63,000	24,000
Subject 11					
Séance No. 32	45,000	11,000	20,000	20,000	13,500
Subject 12					
Séance No. 33	16,000	13,500	17,000	16,000	15,000

TABLE 1 (*cont.*)

	Initial resistance	Resistance just before hypnotism	Maximum resistance in hypnotism	Resistance just before awakening	Resistance after awakening
Subject 13					
Séance No. 34	14,000	13,500	17,500	16,500	14,000
Subject 14					
Séance No. 35	17,000	14,000	19,000	17,500	16,000
" No. 36	12,000	9,500	18,000	15,000	10,500
Subject 15					
Séance No. 37	9,500	8,000	12,600	10,500	10,500
" No. 38	14,500	13,500	17,500	15,500	15,500
Subject 16					
Séance No. 39	14,500	14,000	17,000	15,500	15,000
" No. 40	21,000	19,500	23,000	22,000	21,500
Subject 17					
Séance No. 41	13,500	12,000	16,000	15,000	14,000
Subject 18					
Séance No. 42	58,000	51,000	51,000	50,000	49,000
Subject 19					
Séance No. 43	14,000	17,000	30,000	17,000	17,000
Subject 20					
Séance No. 44	14,000	14,000	14,000	11,000	11,000

10 minutes later. Column 4 gives the maximum reading in the hypnotic state, while column 5 lists the resistance just before awakening. Finally the last column represents the resistance after the subject had been awakened. When the hynotic séance was terminated, the resistance almost always returned to an approximation of the normal. This sometimes was immediate, and sometimes required 4 or 5 minutes. The reading in the last column represents this resistance when it was evident that it was again at a normal waking minimum and was not going any lower.

Subject 1 illustrates very clearly the phenomenon to which attention would be directed. He was an excellent somnambulist who had been repeatedly hypnotized by other operators for experimental purposes. Thus he was already a very good subject. The record of 7 séances which were held are very instructive. In the first we get the normal curve in hypnosis and sleep, no effect until the induction of hypnotism, then a marked rise of resistance with a return to normal at the end of the séance. In séances 2 and 3 the same general result is evident, but in séance 4 we have very clearly marked a phenomenon which is already appearing in 2 and 3. This is a tendency for the initial resistance to start higher owing to the mere presence of the operator and to keep rising before the induction of hypnosis.

We note that in all cases, however, it returns to normal on the subjects' awakening from the trance. In séances 5 and 6 this is even more marked. The subject was always with the operator for about 15 minutes before the experiment began and was getting used to his presence in some way or other. As a result, when we begin the experiment we already find the resistance well up in both cases. At the end of 10 minutes (column 2 of the figures) it has practically reached a maximum, but the hypnotic séance proper has not as yet begun. The subject is wide awake, feels perfectly normal, and says so. The phenomenon is evidently due to some condition of rapport because in séance 7 another individual, whom the subject has never seen before, affixes the apparatus. The writer only arrives on the scene after this is done. Immediately the resistance starts rising, and in the 10 minutes before the séance proper begins it has risen from 15,000 to 44,000 ohms.

Subject 2 was also an excellent somnambulist who had been frequently hypnotized before this series. Here also the reader will notice that at first the rise of resistance occurred only in the hypnotic state. We again see, however, a general tendency for this resistance to go up previous to the séance proper as the subject becomes more used to the operator. This tendency becomes very definite in the sixth and seventh séances with this individual (séances 13 and 14) and again is evidently due to some kind of rapport which becomes established between subject and operator and is not dependent on actual hypnotism. The last séance here again represents a control experiment wherein the subject was attached to the reflex apparatus before the appearance of the operator. Here, as in the last subject, we see no rise in resistance until the operator's arrival, immediately after the contacts have been made. Then the resistance rises very markedly.

Subject 3, in some respects, gives our most interesting example. This subject was not a somnambulist. He could be influenced to the extent of obtaining contractures and forced movements but could not be given hallucinations. On awakening he always had a perfectly clear remembrance of everything and consequently could give a full introspective report. He was hypnotized by a third person and had never been a subject before these experiments. There were thus 3 people present, the subject, the operator, and the writer, who devoted his full attention to the apparatus. It was very interesting to note the progressive depth of hypnosis to which he attained as measured by his resistance remaining up during the hypnotic séance. Thus at first the request to stand up would be followed by a partial awakening and an immediate slump in resistance. Then he could stand up, but any movement of his arms would partially awaken him and the resistance would fall. Finally he could stand up, wave his arms about, talk and open his eyes, while the resistance would still remain normal for the hynotic state.

In the 7 séances with this individual we see the typical results obtained

from the first 2 subjects despite the fact that he was not a somnambulist. They are not, to be sure, as clear-cut, but they are quite evident. We do not get the rise previous to the hypnotic state proper until the fourth and fifth séances (Nos. 19 and 20), but then it is quite marked. Séances 21 and 22 (the sixth and seventh for this subject) mark check séances wherein the usual operator was absent during the entire performances. In No. 21 another operator hypnotized the subject for the first time. He obtained the usual depth of hypnosis and the usual rise in the hypnotic state proper but not the rise before the induction of hypnosis. The same is true of the last séance wherein the writer did the hypnotic work himself. This was his first experiment as operator with this subject, and, notwithstanding the fact that he had been present at all the other séances, he did not get the rise in resistance before this séance as did the usual operator.

A note should here be inserted concerning the height of resistance in these 3 subjects. In the hypnotic state and out of it this phenomenon does not seem due to muscular relaxation as one might expect in the typical rise of sleep. The writer had all these subjects stand up, walk, talk, and in the first 2 he induced various auditory and visual hallucinations without causing a noticeable slump in resistance. Moreover, as has previously been mentioned, the reflex proper vanished in hypnotism as it always seems to vanish or become relatively small with a heightened resistance. W. Smith (4) and many others have noticed this latter point.

Subject 4 was also an excellent somnambulist whom the writer had hypnotized many times before the experiments with the psychogalvanic reflex. He was never operated upon by anyone else. Here we note that the resistance was very high from the moment that the contacts were affixed and that the induction of hypnosis seems to have had no effect whatsoever on it. That they are not the normal resistances, however, is shown by the fact that there is a very definite slump on the subject's being awakened. Here we see a subject who is already at the stage only reached in the first 3 after repeated séances. This is, probably, because he was used to the writer as an operator and was in some sense or other in rapport with him already.

Subject 5 had only one séance. He also was a somnambulist and was brought in by an operator who had repeatedly hynotized him previously. Here, as with Subject 4, we note a high, in fact an abnormally high, resistance at the beginning of the experiment which reaches a fairly constant but lower level as the séance begins, and then slumps very definitely on awakening.

Subject 6 represents an even more interesting case. Here (séance 27) we again see a definite rise before the induction of hypnosis, and yet the operator had not the least effect on the subject. Actually the subject was a somnambulist but had become angry at the operator because in a previous séance—not, by the way, held in connection with these experiments—the operator had made a fool of him. As a result he took part in this experiment

with the mental reservation that, so far as he was concerned, there wasn't going to be any hypnosis. Actually he was perfectly master of himself throughout, broke every test for control except closure of the eyes, and seemed very well satisfied with his performance. For all that, his resistances, for some curious reason, gave the typical hypnotic reaction. The writer was not the operator in this case.

From here on the séances are more after the nature of control experiments and do not give the typical reactions which we have been discussing. The subjects were, in general, hypnotized only once or twice and the hypnotic effects were, in the main, slight.

Thus Subject 7 gives a very typical reaction in hypnosis. He probably went about to Bernheim's third stage. Subject 8, on the contrary, appears to have been just as deeply influenced, but the typical heightening of the resistance is absent. Subject 9 without doubt went to Bernheim's second or third stage and gives the result to be normally expected. Subject 10, himself a very good operator and this time acting as a subject, had been repeatedly hypnotized before. The results are always slight, but the effects on the resistances are very typical. Subject 11 was almost wholly uninfluenced, as were, indeed, all the subjects up to and including Subject 19. Subject 20, however, offers a rather puzzling exception. According to his introspective reports, he went to a rather deep stage, verging on somnambulism. Yet, so far as his resistances are concerned, they remained practically stationary throughout the entire performance.

The results of these experiments lead to the following tentative conclusion: In hypnotism, as in sleep, the gross resistances tend very definitely to increase. This may give us some really objective measurement of the depth of hypnotism, a need very keenly felt, for instance, by Babinski (1) who claims that at present it does not exist.

With these heightened resistances the reflex tends to disappear, becoming less pronounced as the resistance increases, a phenomenon noted by Smith (4) and others.

Most curious of all, however, is the fact that once a good subject is more or less used to a certain operator, his resistances will tend to increase with the mere presence of this operator. This increase appears to be quite distinct from any condition resembling sleep or the hypnotic trance as it is generally understood.

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A SIMPLE STATISTICAL METHOD FOR PSYCHOLOGY

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The following study of learning as it occurred in a substitution test shows how the data of speed and accuracy may be treated in a new way more simply and may be more clearly summarized than is the case when the traditional methods of correlation are used.[1]

Adelbert College freshmen in the fall of 1927 were given the Johns Hopkins Substitution Test, Form 3. In this test 2 pages containing 6 different geometric designs (a circle, square, triangle, etc.) were presented with an appropriate place for a check mark on each design. The 2 pages differed in that at the top of one was a "key" while the second was used without the "key." The students were required to put the appropriately placed check mark on each design, taking each one in the order of its occurrence. The test was given in 6 time units of 1 minute each. For 1 minute the student marked the keyed sheet, for a second minute the unkeyed one (without reference to the key or the keyed sheet), for the third minute the keyed sheet, and so on, alternately. These 6 periods are designated hereafter as *A, B, C, D, E,* and *F,* respectively. The *A, C,* and *E* periods are, then, those in which the student had the "key" before him, and the *B, D,* and *F* periods are the periods with the unkeyed performance. The latter serve as indices of the amount learned.

The test was given to 258 students. The papers then were graded and the totals of all attempts, rights and errors, of each man were recorded for each period. The omissions, which for all students in all periods were negligible, were considered as errors. An examination of the papers disclosed that quite a number of students failed in some way or other to follow

[1]Dunlap, in his Powell Lecture in Psychological Theory at Clark University, April 21, 1925, states our problem nicely as follows: "Individual psychology, which has been carried away by the mathematical fascination, and which has made its mental measurements so largely matters of correlation, can regain its balance and justification by returning to the fold of experimental psychology and profiting by its experiences, disillusionments, and achievements."

the instructions. The major offences were: (1) omission of initials at the end of a period's work (placed there in order to distinguish one period from another); (2) marking only the first or only the second sheet; (3) marking all designs in the same place; (4) picking out only one design and marking it on both pages; (5) too high a percentage of errors in the first period (above 50%); and a few others. The total number of papers ruled out reached 76, or 29.4%, leaving 182, or 70.6% of valid papers. All ruling out was done before any computations were made.

The problem which interested us was the determination of the nature of the increase in speed and decrease in error and the relation between the two.

The orthodox statistical procedure shows the general tendency to be toward increase in speed and decrease in error (which, of course, would be expected).

For reasons which are obvious later the periods of importance seemed to us to be the *B, D,* and *F* periods, in which the subjects marked the unkeyed page.

The means of all attempts (that is, rights plus errors) of all subjects in different time units were as follows: $B=13.9$; $D=20.5$; $F=25.9$. The modes have the following values: $B=11.5$; $D=20.5$; and $F=17.8$. Thus we see that the bulk of the testees does not rigidly conform to the "normal distribution" of speed. From this discrepancy between the mean and the mode we could state *a priori* that the standard deviation from the mean will tend to become larger with each consecutive period, and an empirical determination shows σ of the mean to be for $B=6.36$; for $D=8.07$; and for $F=10.79$.

The values of the median ($B=13.3$, $D=20.3$, $F=24.7$) follow closely the movement of the mean.

Summarizing these measures of central tendency, we can say that there is a trend toward increase in speed; that speed curves in different periods vary differently; and that there are different tendencies within each speed curve. The problem still remains as to whether there is anything like constancy in these tendencies.

The mean of the absolute number of errors shows a trend toward decrease. The values of the means of the errors run as follows, $B=7.1$, $D=5.8$, $F=3.9$; while the modes of the absolute number of errors are, $B=8.3$, $D=6.3$, and $F=0$. The values of σ of the mean are $B=4.35$, $D=4.23$, and $F=3.81$.

Thus the central tendency of the absolute number of errors shows a very irregular decrease in errors, if judged from the standpoint of normal distribution. There still remains the problem of the relation between the speed and the accuracy.

At this point one may try the Bravais-Pearson correlation methods. Considering only the *number of attempts* made by different individuals in dif-

ferent time units, we find the values of r to be as follows: for B and D, $r=.49\pm.04$; for B and F, $r=.47\pm.04$; and for D and F, $r=.73\pm.02$. From this we can conclude that there is more uniformity in the distribution of the total number of attempts made by the same individuals with respect to the D and F periods than with respect to the B and F or B and D periods. Enlightening as these coefficients are, a superficial inspection of a quickly drawn scatter diagram would show just as much.

Nor does one get much illuminating information about the relation between the speed and accuracy by the accepted methods of correlation. The values of r for the speed, i.e., the total number of attempts, and the absolute number of errors which each student made in each period are as follows: for B, $r=.59\pm.03$; for D, $r=-.15\pm.05$; and for F, $r=-.35\pm.04$.[2] It is interesting to notice here that the values for r for speed and accuracy in A, C, and E periods are all positive, being as follows: for A, $r=.37\pm.94$; for C, $r=.11\pm.04$; for E, $r=.09\pm.01$. The change in sign of r to minus in D and F tells us, perhaps, that the actual advance in learning takes place after the subject checks himself once on his achievement. But this is hardly worth the effort put into the computation of r's. The orthodox correlationist way of treatment is, perhaps, not as bountiful as its fame makes it.

As a matter of fact, there are possibilities of a simpler and more informative way of treating the results of psychological tests. For the present we may limit ourselves to group tests only and consider a group test as a cumulative performance of a number of individuals. That is, it may be regarded as a sum of their efforts put into the test. A performance, then, as a whole may be expressed as a logical summation or resultant of the work of the group. Different individuals work with different rates of speed and turn out different qualities of work. Some classes contribute more than others to the total accuracy of the entire performance; others pile up errors and lower the standards of accuracy of the group as a whole. But what is the *standard of accuracy of a group* as it shows itself in the performances? We may conveniently define it as the total number of errors divided by the total number of attempts. The standard of accuracy of a group as a whole is then a fixed and stable quantity and, therefore, may be used as a point of reference for measuring the deviation of individual performances with respect to accuracy. But the standard of accuracy so derived leaves out of consideration the variations in speed within the group, an element of importance in any performance.

A speed-accuracy measurement needs a point of reference *sui generis*. It must be fixed and stable for the given performance; it must be in itself a certain property of performance and in no case an extraneous norm intro-

[2]It should be noticed here that before any significance should be attached to even such striking coefficients as these one needs to take into account the spurious correlations involved.

duced or devised *ad hoc*. In other words, to be of any real significance at all this speed-accuracy point of reference should be for each performance "its own," independent of other performances and any extraneous facts.

For this speed-accuracy measurement we may introduce new sets of values. Instead of considering the absolute number of attempts made by a particular individual in a particular time unit as an all important figure, we may, without doing violence to our data, take the performance of all the subjects and consider in each unit of time the ratio of the number of attempts made by a given speed class[3] to the total number of attempts made by all the subjects during that particular time unit. Then we may treat the errors in exactly the same way by finding the ratio between the sum of the numbers of errors made in the particular speed class and the whole number of errors made in that time unit. We may express these ratios in percentages. If it is done for both attempts and errors, we may plot on one axis of coordinates the cumulative percentage of attempts starting with the smallest values and on the other axis the cumulative percentage of errors starting with the smallest values. The curve thus plotted is known in statistics as the Lorenz curve. Figure 1 shows its appearance for the *A, C,* and *E,* and *B, D,* and *F* periods, respectively. The cumulative percentage of attempts is plotted on the ordinate and the cumulative percentage of errors is plotted on the abscissa. If, throughout the entire group the subjects making a given percentage of the whole number of attempts made the same percentage of the whole number of errors, the plotted locus of

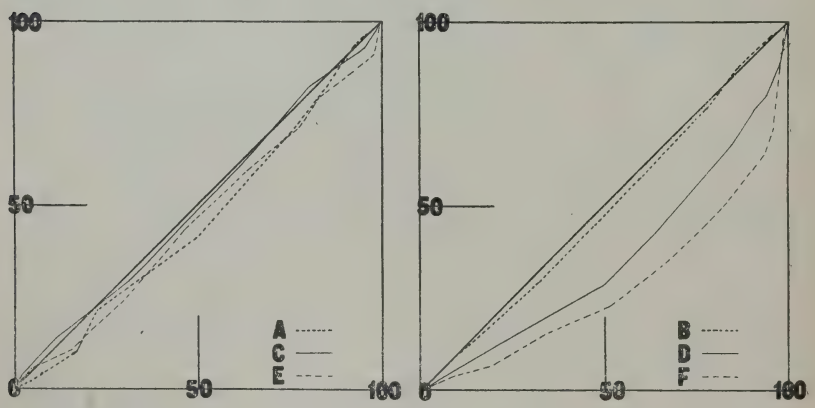

FIGURE 1

[3]For statistical convenience the speeds of work were divided into classes with a range of three attempts for each class. Thus in period *B,* for example, we have 12 speed classes in a range of 1-36 attempts.

points would follow the "line of equal distribution," the heavy solid line shown in Figure 1 running at an angle of 45° from zero.

It is obvious that with the "key" before the subjects the equality of these percentages is about what can be logically expected to happen. But in *B, D,* and *F* periods one notices a marked difference. In the *B* period, for example, we find that 52% of the errors are made by the subjects who have done about 50% of the marking. In the *D* period, however, we find that 70% of the errors are made by the subjects who have done 50% of the marking. And in the *F* period more than 80% of the errors are made by the subjects who have done only half of the total "work" for that period.

With this principle in mind we can proceed to a study of *dispersion of speed and accuracy* through the various speed classes of different periods. In Figure 2 this has been shown for the values which we have designated as *K*. On the abscissa appear the different speeds of work. In period *B,* for example, the number ranges from 1-36 and is shown by the heavy dotted line. The zero line, around which the values are plotted, represents the line of equal distribution of attempts and errors and corresponds to the heavy solid line in Figure 1.

To get any particular point on this curve, the ratio of the number of errors made by the subjects in a particular speed class to the total number of errors made by the group during the given period is subtracted from the ratio of the number of attempts of the same speed class to the total

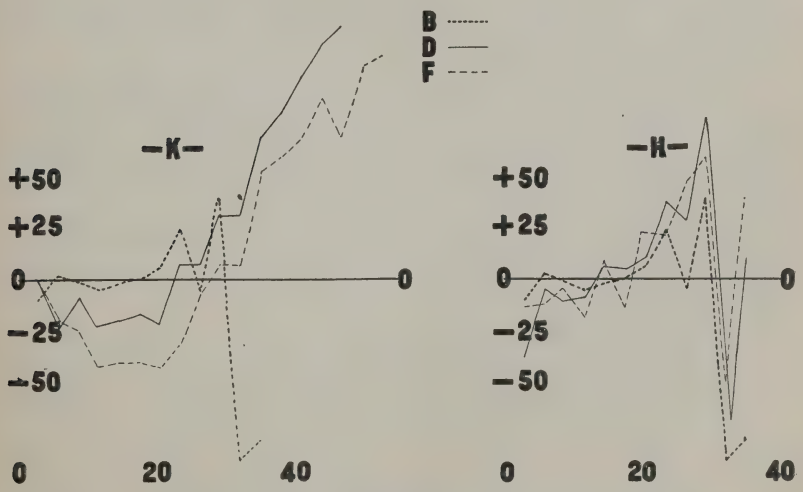

FIGURE 2

number of attempts of the entire group during the same period and the result divided by the frequency in that class, i.e., by the number of students each of whom marked a given number of designs. If the value is negative, the relative part played by the average individual of that speed class was greater in the production of errors than in the work attempted. If the value is positive the errors were few in proportion to the percentage of the work attempted by that speed class in that period.

Thus we see from our *K* curve that the slow speed classes are responsible for a relatively large proportion of errors, while we find a relatively small number of errors in the high speed classes during periods *D* and *F*. And we see, further, that there is for a short time, proceeding from the slow to the fast workers, an increasing culpability relative to their work output, and that in the *B* period this culpability is most marked in those who work most rapidly and is least marked in those who worked most rapidly during the *D* and *F* periods. The lines of regression could give us the general trend of these changes, but only after determining r and σ values.

In our *K* study (see Figure 2) we considered the speed classes in each period irrespective of the permanency of subjects which compose them. Thus certain subjects who happened to be in the same speed class in the *B* period may be (and very frequently are) in different speed classes in the *D* and *F* periods.

In order to study the general tendencies in performance of each individual in so far as they manifest themselves from one period to another, we need a different base for classification, a base which would hold fast the position of an individual subject and allow us easily to bring together his performances in different periods.

It is obvious that for this purpose *any* trait in performance which is sufficiently pronounced to be discriminative in any period would serve. Choosing it as the base, we can gather together the performances of individuals which compose the fixed classes obtained on this basis and thereafter deal with the variations of the same class-aggregate of individuals in subsequent periods. Among other possibilities of such "holding-fast-the-individual" base of classification, we chose the speed classes in the *B* period simply because the classification along the speed line had already been made. Applying now the elaboration of the principle of the Lorenz curve to the data thus redistributed we derive the value *H* (Figure 2).

From this point the procedure for computing the value *H* is identical with that for computing the value *K*. Since we chose the speed in *B* as the base of fixed classification of individuals, the values of *H* and *K* are the same for the *B* period. But they differ substantially in subsequent periods *D* and *F*.

The curve of *H* values shows that the individuals who start out making few errors with respect to the amount of work done during the first period

continue in subsequent periods of work to make few errors in proportion to the work output.

This leads us to an examination into the consistency of speed and of accuracy, respectively. So we derive 2 more values, *Q* and *T*, which are shown in Figure 3.

In the study of *speed* (*Q*) we concern ourselves only with the number of attempts. If we classify the entire group according to the number of attempts in a given period, we can then trace the trends of different speed classes toward increase or decrease of "original" speed in subsequent periods. In order to obtain the *Q* value the original speed was taken to be the speed in the *B* period. The ratio of the sum of the number of attempts which the members of any one of the *B* speed classes made in the *D* period to the total number of attempts made by the group in the *D* period is subtracted from the ratio of the attempts of that same class in the *B* period to the total number of attempts in the *B* period, and then the result divided by the frequency in the class. An identical procedure takes place in finding the deviation of these ratios for the *F* period. We see a surprisingly close correspondence between the *BD* and *BF* curves in the *Q* study. That is, as we proceed from the slow to the rapid individuals in the *B* period, their

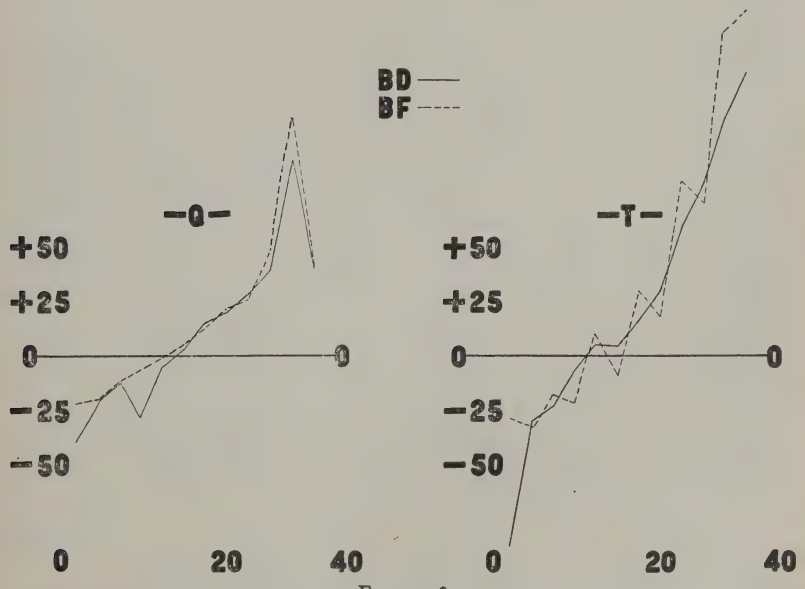

FIGURE 3

comparative slowness in the *D* period and in the *F* period remains about the same.

But if we turn to accuracy relations within the same periods, we find a different situation. The derivation of the value *T*, the measure of accuracy's trend, is identical in principle with that of the *Q* value. The ratio of the number of errors of a particular fixed class (as grouped in *B* period) in *D* to the total number of errors in the *D* period is subtracted from the ratio of the number of errors of the same class in *B* to the total number of errors in the *B* period, and divided by the class frequency. This procedure gives us the *BD* curve of *T* values. In an identical way we derive the *BF* curve of values of *T*.

An examination into the character of these curves shows that there is no consistency in the increase of accuracy of different individuals. While the increase in speed is constant, the increase in accuracy is faltering and the curve is correspondingly zigzagging. From this we see that the accuracy does not show the same uniform changes throughout the test with respect to different individuals that the speed shows.

Table 1 with the legend shows how simply the values discussed here may be derived.

TABLE 1

Class Interval	V	f_B	a_B	$\frac{a_B}{A_B}$	e_B	$\frac{e_B}{E_B}$	K_B
1-3	2	3	6	0.2	6	0.5	—0.10
4-6	5	19	95	3.8	46	3.5	+0.02
7-9	8	25	200	7.9	106	8.2	—0.01
10-12	11	41	451	17.9	259	20.0	—0.05
13-15	14	27	378	14.9	199	15.3	—0.02
16-18	17	20	340	13.5	175	13.5	±0.00
19-21	20	24	480	18.9	228	17.6	+0.06
22-24	23	13	299	11.8	112	8.6	+0.25
25-27	26	7	182	7.2	98	7.6	—0.05
28-30	29	1	29	1.2	11	0.8	+0.40
31-33	32	1	32	1.3	29	2.2	—0.90
34-36	35	1	35	1.4	29	2.2	—0.80
Totals		182	2527	100.0	1298	100.0	

This table shows the method of computation for *K* values in period *B*. These are represented by the heavy dotted line in Figure 2.

Class intervals indicate the number attempted, including rights and wrongs.

V—midpoints of class intervals—shown in Figure 2 on the abscissae as from 0-40.

f—number of subjects in each class; f_B—number in *B* period

a—number of attempts made by a class as a whole; a_B in the *B* period.

(All subscripts represent periods.)

A—sum of a's: $A_B = 2527$.

e—number of errors made by a class as a whole.

E—total number of errors; $E_B = 1298$.

$\frac{a}{A}$—is given in percentages, shown cumulatively for each period on the ordinates of Figure 1.

$\frac{e}{E}$—is also given in percentages and shown cumulatively on the abscissae of Figure 1.

$$K = \frac{\frac{a}{A} - \frac{e}{E}}{f}$$

For derivation of K values all subscripts must remain the same, e.g.

$$K_F = \frac{\frac{a_F}{A_F} - \frac{e_F}{E_F}}{f_F}$$

Notice that f_F here refers to the speed classes in F period and must not be confused with the speed classes in B or other periods.

Now in order to derive H, Q, and T, we hold fast the individuals in each of the B speed classes. Hence, holding fast these individuals, the frequencies remain the same and therefore we need a new set of symbols.

s_B—number of attempts made by a given class as a whole in B period.

s_D—number of attempts made by those same individuals in D period.

S_B—sum of s_B's in B period.

w_B—number of errors made by any class as a whole in B period.

W_B—total number of errors in B period.

Notice here that s_B is identical with a_B but s_D is *not* identical with a_D. Similarly w_B is identical with e_B but w_D is *not* identical with e_D.

$$H = \frac{\frac{s}{S} - \frac{w}{W}}{f_B}$$

f retains the subscript B for derivation of all H values; the other subscripts change from period to period, but for any given period all subscripts other than f's should be the same, e.g.,

$$H_D = \frac{\frac{s_D}{S_D} - \frac{w_D}{W_D}}{f_B}$$

The derivation of Q and T are obvious from the text, e.g.,

$$Q_{BD} = \frac{\frac{s_B}{S_B} - \frac{s_D}{S_D}}{f_B} \quad \text{and} \quad T_{BD} = \frac{\frac{w_B}{W_B} - \frac{w_D}{W_D}}{f_B}$$

SUMMARY

This simple way of treating the data of psychological tests has several advantages, and it is particularly desirable when contrasted with the Bravais-Pearson correlation method, i.e., the orthodox product moment formula. It is simple in the way of computation; all of the arithmetical operations involved are elementary. The results obtained show clearly relations within the data which are not so easily shown in other ways, and do not depend for this on the hypostatization of intervening concepts, mathematical, or otherwise. The results are only slightly affected (if at all) by the non-linearity of correlation which plays havoc with the r. The one new, basic assumption is simple and logically acceptable, i.e., that a group performance may be regarded as the logical sum of the individual effort put into the work. It makes possible analytically to treat the different results as shown in the raw data in many different useful ways, for example, in the Q and T studies. Last but not least, it is an approach to the introduction into mental tests of a set of stable and significant mathematical values which are consonant with the requirements of experimental psychology.

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SUGGESTIONS FOR IMPROVED FORMS OF STEADINESS TESTERS

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In the majority of laboratory investigations on the nature of manual steadiness, the conventional "steadiness block" has been employed as the measuring device. In an experiment[1] conducted by the writer which involved the steadiness responses of 50 male college students, the familiar Stoelting steadiness block was found to be an inadequate device for ascertaining degree of manual stability. This form of steadiness block consists of a metal plate perforated with 9 holes graduated in a descending order of size. According to the function of this instrument, manual steadiness is conceived not as the maintenance of a fixed position in space, but rather as the ability of the subject to avoid contact with the circumferential limits of each suc-

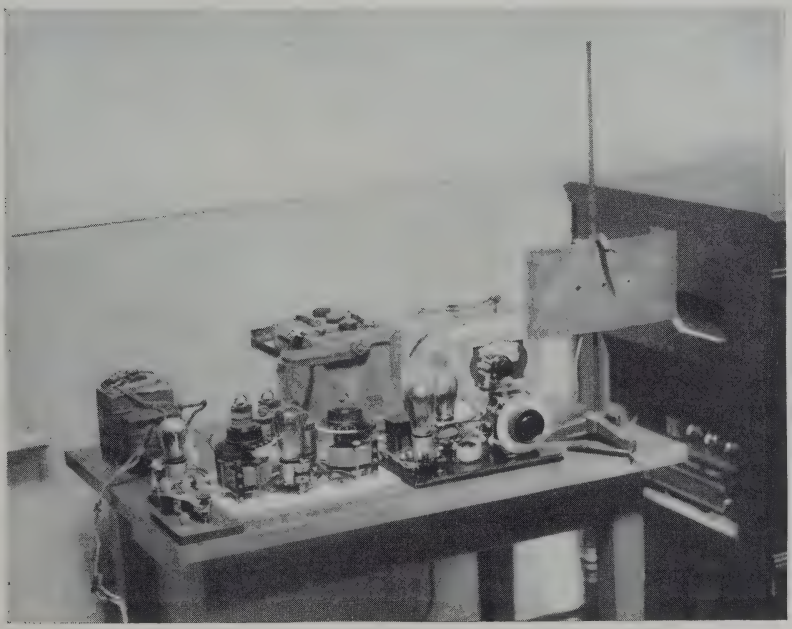


FIGURE 1

PHOTOGRAPH OF THE OSCILLATOR CONTROL

On the left of the table are shown the transmitting oscillator, thermionic rectifier, and power transformer, while on the right are located the detector unit, steadiness plate, and power amplifier.

[1]Conducted at Lehigh University.

cessive aperture. Since degree of fluctuation from a given point in space varies with the circumferential limits of stylus confinement, the total number of contacts made by any subject is not an accurate nor true index of manual steadiness. It has been observed that, in general, more pronounced movements of the hand occur in the case of the first large aperture than in the case of the third relatively smaller hole. The method of recording, however, does not take into account these more extensive fluctuations in the first instance.

Three principal methods of recording stylus contact have been employed, no one of which affords a true index of hand or finger movement. In the first method, the number of contacts is recorded by some form of electromagnetic counter. This method has been found unreliable for a number of reasons. The armatures of practically all of the counters used for this purpose are activated by magnetic coils of low resistance winding. In such instances where the current consumption is large, despite the use of a shunt inserted condenser, frequent arcing not only modifies the response of the subject and gradually alters the size of the aperture, but it oxidizes the metallic surfaces of the stylus and plate so that in due time the counter fails to respond to actual contacts as a result of increased circuit resistance. Moreover, through such unavoidable arcing and the resultant variation in conductivity of the metallic surfaces, a single contact of the stylus marked by some slight movement on the circumferential limits of the aperture may result in an erroneous record of as many as 10 or more distinct contacts.

In spite of rigid instructions to the contrary, the subject will frequently allow his stylus to rest against the edge of the hole for a number of seconds. This practice may be indulged in unconsciously or else deliberately for the express purpose of reestablishing equilibrium. Ofttimes indulgence in this practice is not apparent to the experimenter. Unfortunately, the ordinary electromagnetic counter affords no indication of such violation of the test instructions. In instances where hand-movement tends to persist in a given direction, it is frequently impossible to avoid fixed contact over a period of several seconds. For this reason, the smaller holes of the steadiness block do not afford a genuinely diagnostic record. In 52% of the total number of trials for both right and left hands undertaken by the previously cited group of 50 subjects, a marked improvement in score was shown in the eighth hole over the seventh, and in the ninth over the eighth. According to test design, one might readily expect an increase in the number of contacts with diminution in the size of the aperture. The reverse condition, which actually follows, is largely attributed through careful observation to the aforementioned tendency to fix contact over several seconds.

In the second method, the number of contacts is recorded on a kymographic drum by means of an electromagnetic pen. This method is in certain respects superior to the method of employing a simple magnetic counter. Besides affording a means of relating contact incidence and contact pattern with the time factor, the kymographic record readily reveals instances of contact fixation. An electromagnetic pen wound to 100 ohms resistance

materially assists in eliminating arc interference, since only a small amount of current is necessary for activation. By using a condenser of proper capacity shunted across the stylus and steadiness plate, it is possible to reduce sparking effects to an almost imperceptible minimum. While the kymographic method presents these few advantages, the method does not, however, eliminate some of the more serious discrepancies noted in the use of the magnetic counter. Slight movement of the stylus on the edge of the aperture may likewise produce an excess of contacts on the kymographic drum. This method of recording likewise does not afford any indication of orientation of hand movement. In general practice, the kymographic method of recording has proved unpopular because tabulation of the records obtained is a tedious and laborious undertaking and because of the several difficulties frequently encountered in attempts to effect a correct interpretation of the results.

In the third method, the observer uses a telephone receiver in counting the number of contacts made within a given period of time. The telephone receiver, activated by a single dry cell, is connected in series with the steadiness plate and stylus. Contacts of the stylus on the plate produce a coincident succession of clicks in the telephone receiver which are mentally summated by the observer. While this method of recording introduces certain conveniences, it is less reliable than either the counter method or the kymographic method. The telephone receiver, depending upon the tension status of the diaphragm and the amount of current employed, may frequently produce double clicks for single contacts. Since the magnetic coils of the ordinary receiver are extremely sensitive to slight fluctuations in current, a very slight movement of the stylus on the plate may produce a rapid succession of clicks which are not distinguishable from the usual contact clicks. The chief disadvantage of this method is that it provides no objective and precise record of the subject's performance. Computation of the number of contacts through observation of a succession of clicks demands special training in auditory recognition and discrimination. The slightest flag of attention on the part of the experimenter may vitiate against an accurate score. Moreover, it is impossible to effect an accurate count in instances when the clicks follow an unanticipated rapid succession. This condition is found to exist especially in the case of the smaller apertures. In using the telephonic method of recording, it has been experimentally demonstrated that in diagnosing the performance of a given individual, different observers vary in accuracy and that the same observer varies in accuracy with different subjects.

If steadiness is conceived as the maintenance of a fixed position in space, then the methods thus far described are inadequate to measure accurately such a condition. In order to obtain an objective record of the degree of fluctuation from a given point in space, the 2 following methods are suggested: (1) deflected-light-beam method, and (2) capacity-coupled oscillographic method.

Of these 2 methods, the deflected-light-beam method is simpler both in

conception and in operation. Three essential pieces of apparatus are required in this scheme: (1) a point-light operating on a direct-current output; (2) a specially constructed stylus with a terminal foco-deflecting mirror; and (3) a continuous-film camera such as is frequently employed in string galvanometric research. The light from the point source is reflected as a pin beam by the stylus mirror on a moderately sensitized plate. If radius of hand movement and complete movement pattern are desired, a stationary plate exposed for a number of seconds will provide this result. If a record of the horizontal motion pattern is desired, this can be obtained by revolving the film in a vertical direction. Conversely, a record of the vertical motion pattern can be obtained by revolving the film in a horizontal direction. The subject is instructed to maintain a fixed focus of the light projection on the center of the camera aperture. While this method will provide a picture of actual hand movement, unless the conditions are very carefully controlled, the scheme becomes impractical. One of the chief disadvantages is that the subject must perform in a darkened room. Since the radius of movement on the sensitized plate is related to the distance between the plate and stylus, it is exceedingly important in comparative studies of hand movement to keep this distance as constant as possible.

In the second suggested method, 2 thermionic valve oscillators are employed in conjunction with an amplifier, steadiness plate or ring, stylus, and dynamic recording pen. The factor utilized in this method is that of the current gradation that results from the difference frequency generated by the capacity coupling of two simultaneously operating oscillators.[2]

The transmitting oscillator is operated from the usual 60-cycle, 110-volt synchronous alternating current circuit. A power transformer connected in circuit with 2 30-henry choke coils and 2 fixed condensers of 2 microfarads capacity each, supplies the thermionic rectifier UX 216-B and the oscillating valve UX 210 with the necessary filament and plate voltages. The thermionic rectifier UX 216-B is preferred to the more recently manufactured rectifier UX 281 since the thoriated tungsten filament of the former tube is not readily exhausted by an overload of heating voltages, whereas the oxide filament of the latter is quickly exhausted by slight overloading and cannot be rejuvenated through "cooking." The thermionic rectifier is found to be superior to the electrolytic type since it has no leakage, its internal capacity is practically zero, and its operating characteristics, independent of changes in temperature or load, remain constant. Thermionic rectification is chosen to avoid the ultimately greater expense entailed in the use of a large bank of B-batteries for supplying the high plate voltage of the oscillating tube. The transmitting oscillator is of the double functioning tuned-plate tuned-grid type. The variable condenser in the plate circuit is inserted for the purpose of vernier adjustments. Pilot socket-type short wave low-loss coils function

[2]The writer wishes to thank Mr. Herman Lehrer for his cooperative assistance in the construction of the oscillator units and for his kind loan of much apparatus which made possible the present investigation.

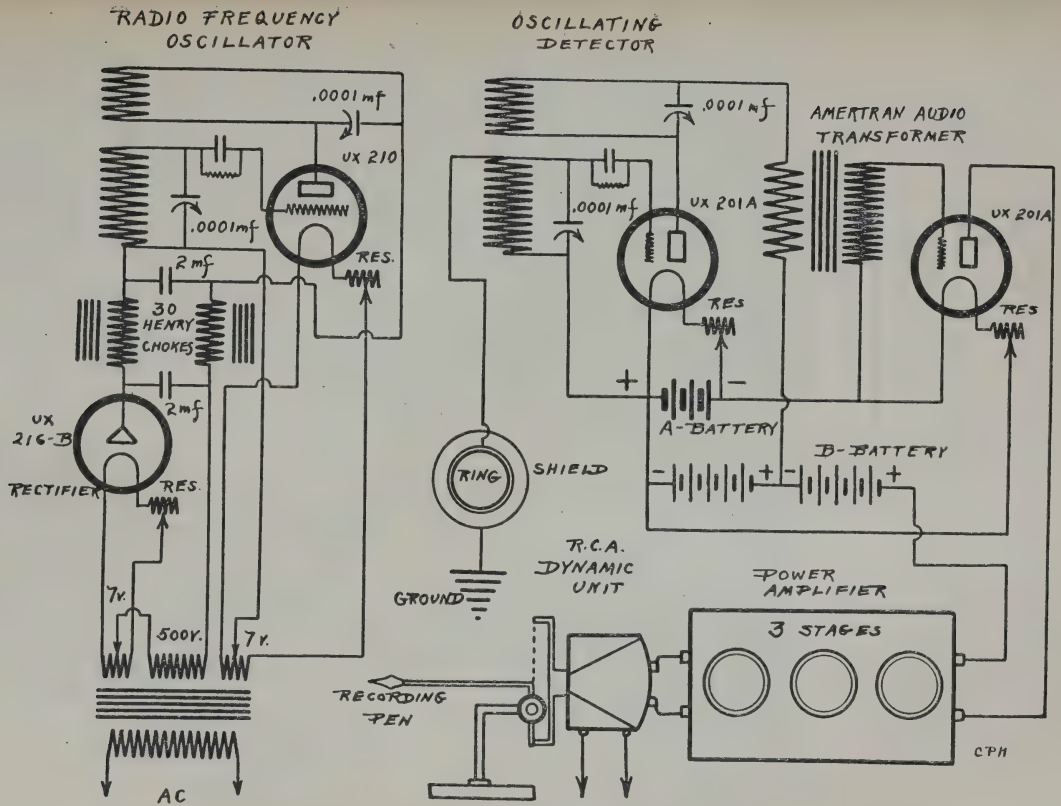


FIGURE 2
COMPLETE ELECTRICAL HOOK-UP FOR OSCILLATOR CONTROLLED STEADINESS TESTER

as the primary and secondary inductances in both the transmitting and detector oscillator circuits.

The detector oscillator is also of the tuned-plate tuned-grid type. Vacuum tube UX 201-A is used in both the detector stage and in the succeeding stage of audio amplification. The output of the audio amplifier is connected with an auxiliary power amplifier utilizing 3 additional stages. Since the power amplifier is supplied with current directly from the light socket of a 110-volt 60-cycle alternating current line, 2 UX 216-B valves are employed for rectification, while a ballast tube is inserted for filament protection. Although thermionic rectification does not provide pure linear reduction, through the introduction of a balanced potentiometer circuit, cyclic interference in the final stage of amplification is reduced to a considerable degree.

The output of the power amplifier is connected in circuit with an RCA dynamic unit, the cone of which activates at a right angle to the line of armature vibration the writing point of a specially constructed recording pen. For a diagram of the complete hook-up, see Figure 2. The celluloid writing point of the recording pen is adjusted against the smoked drum of an ordinary kymograph rotating at high speed.

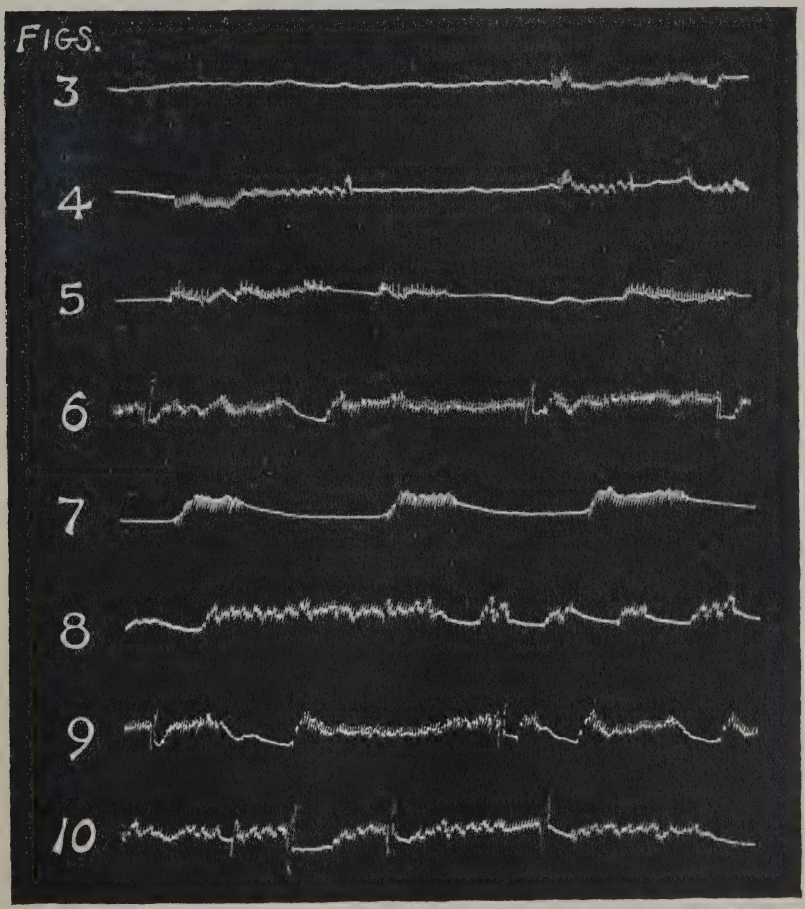
Two forms of grid electrode may be employed. The first consists of a 2-inch strip of 1/32-inch stock brass perforated with 5 apertures of diameters in the following descending order: 5/8, 1/2, 3/8, 1/4, 3/16 of an inch. This inner strip is attached to, but carefully insulated from, an outer brass plate perforated with apertures of the same diameters and arranged in the same descending order of size. The outer plate serves as a ground shield and protects the grid electrode from gross body capacity. In order to obtain a record of hand stability it is essential that the stylus be projected through both the outer and inner apertures, since the capacity coupling between the stylus which is grounded through the subject's body acting as register and the outer plate which is directy grounded is zero.

In actual testing, this form of steadiness plate possesses a number of disadvantages. Considerable sensitivity is sacrified through the fixed capacity coupling between the grid electrode and the ground shield. As the ground shield approximates a position coincidently parallel with the grid electrode, sensitivity is proportionately decreased. While separation of the ground shield from the grid electrode provides greater sensitivity, stylus manipulation becomes more difficult to effect. In order to eliminate this difficulty, another form of grid electrode was employed. This consists of a simple brass ring 3/4 of an inch in diameter shielded by a concentric coil of wire suspended 1/2 inch above and before the grid electrode. It is extremely important that the grid connection be as short as possible and connected perpendicularly to the diameter of the electrode. Since a measure of the degree of fluctuation from a given point in space is desired and not the actual number of contacts made against the sides of the apertures differing from each other in size, a single fixed aperture of a specific diameter serves to standardize conditions.

In order to obtain zero difference frequency, the detector oscillator is

tuned to the same frequency as that of the carrier wave of the transmitting oscillator by means of the variable condensers in the grid and plate circuits. This zero difference frequency condition is effected when the stylus capacity coupling is exactly central. Unfortunately, during operation a perfect zero frequency condition in the output of the amplifier does not result, since the alternating current cycle is not completely filtered through the thermionic valves and potentiometer circuit. Through the use of pure direct current for filament and plate supply, this condition might readily be eliminated.

If both the transmitter and detector are tuned to oscillate say at 1,000,000 cycles per second (300 meters), the recording pen will indicate a simple



FIGURES 3-10

linear kymographic tracing. If the stylus deviates from central position, capacity coupling is varied and heterodyning results. As the stylus approaches the grid electrode, the deviation of the detector oscillation from the carrier wave gradually widens, resulting in an increase of the heterodyne frequency with a corresponding increase in the terminal energy output. Since the type of filter system employed in the detector amplifying circuit was inadequate to eliminate completely the alternating current pulsations, cyclic interference during increased capacity coupling was enhanced and vitiated against a simple linear curve. In the illustrative figures presented, if the basal axis is traced through the recorded pulsations, a curve of stylus fluctuation is obtainable.

Figure 3 illustrates the type of curve that results from a relatively steady suspension of the stylus in approximately central position. The latter part of this curve indicates the tendency to become increasingly unsteady. The central portion of the curve in Figure 4 indicates that the subject was able to maintain fixity of stylus position, whereas the curve extremes indicate considerable variation in stylus movement. Figure 7 illustrates the type of record that is obtained when the stylus is drawn away from central position towards ring periphery and then returned. When the stylus touches the electrode, the recording pen makes a decisive spurt as indicated in Figures 6, 9, and 10. The other figures serve to illustrate the forms of movement fluctuations that are obtained from different subjects.

The apparatus and hook-up herein described must not be considered the acme of instrumental perfection, but rather a preliminary suggestion for further investigation. With complete elimination of cyclic interference and further refinement of the various circuit connections, the writer believes that the method herein described is adequate to measure stylus movement.

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MULTIPLE-PLATE TAPPING BOARDS

Christian Paul Heinlein

While the simple rectangular shaped alternate-plate tapping board has been frequently employed as a diagnostic device in studies of handedness, no serious attempt has been made to study manual coordination by means of the triple- and quadruple-plate tapping boards. In order to demonstrate variation in performance between the right and left hands following the adoption of different tapping patterns, 3 circular tapping boards were constructed by the writer.

The base of each tapping board is constructed of 3-ply sectioned maple, 7/8 of an inch in thickness and 10 inches in diameter. The tapping plates are fashioned from 3/32-inch stock medium hard sheet brass. The plates are fastened to the baseboard by means of screws and narrow maple strips

which serve as sector divisors. The sector divisors are 5/8 of an inch wide and 3/16 of an inch above the surface of the plates. The fixation disc, made of circular bakelite 1-1/4 inches in diameter and 5/16 of an inch in thickness, is fastened to the baseboard by a screw which passes through its center. Figure 1 illustrates diagrammatically the construction of the quadruple-plate tapping board. Two views are shown—top view and side view. It will be noted that each plate is an independent electrode connected to a

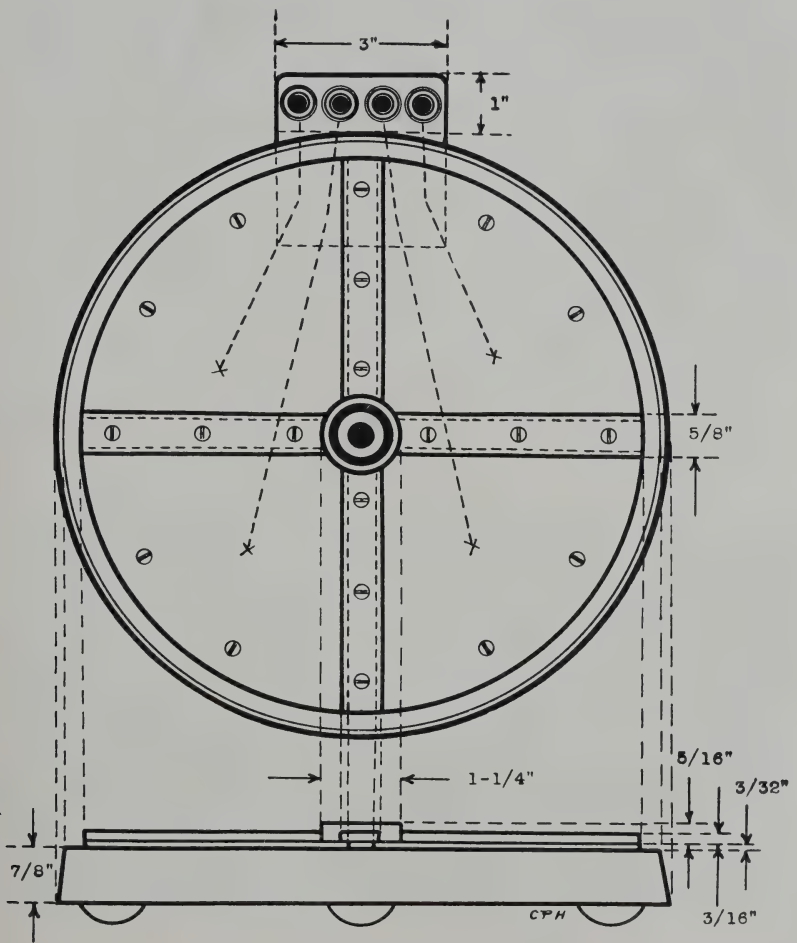

FIGURE 1
CONSTRUCTION OF THE QUADRUPLE-PLATE TAPPING BOARD

separate binding post. In the top view of the diagram, the dotted lines which run parallel with the sector divisors indicate the concealed edges of the 4 plates which are fastened to the baseboard by means of the divisors. The baseboard rests upon rubber cushions which assist in reducing noise and in absorbing excess vibration. Figure 2 shows the 3 complete tapping boards ready for use.

In obtaining tapping records, the kymographic method of recording was found to be the most satisfactory method. In preliminary experimentation, counters were employed as the recording devices. The counter is unsatisfactory because it requires considerable current for activation. Whenever the current consumption is high, arcing occurs at the point of contact. Arcing can be partially but not completely eliminated by means of a suitable shunt condenser. Continued arcing produces a dielectric corrosion on the surfaces of the plates and stylus which prevents circuit completion. The use of high-resistance electromagnetic pens eliminates visible arcing and the consequent rapid film oxidation of the contact surfaces. When using low-resistance wound counters in series with a condenser-shunted 6-volt current supply, the tapping plates were found unfit to use after 10 hours of operation. Approximately 15% of the stylus contacts would not activate the counters. On the other hand, when high-resistance wound electromagnetic pens were employed, the surface conductivity of the plates remained perfect after more than 50 hours of laboratory use. In using counters it is necessary to repolish the plate surfaces at frequent intervals. In using the high-resistance wound pens, no repolishing of the plate surfaces is ever necessary since the current

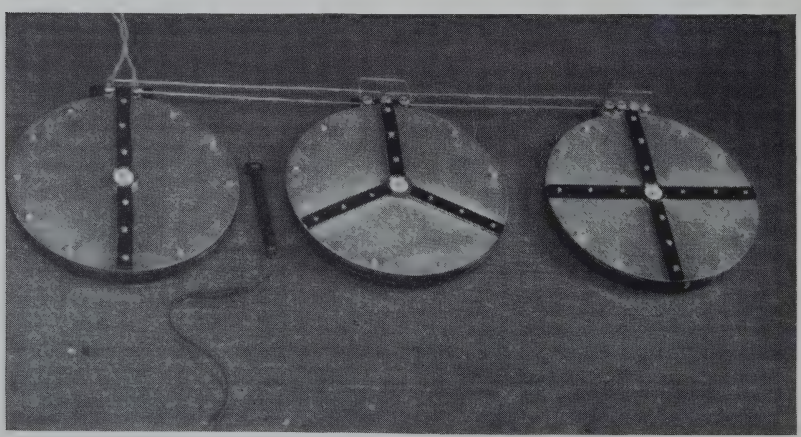

FIGURE 2
THE BANK OF MULTIPLE-PLATE TAPPING BOARDS READY FOR USE

consumption is very low, arcing is practically eliminated, and the mere stroke of the stylus against the plate assists in maintaining a lustrous transmitting surface.

Some experimenters object to the use of kymograph in recording tapping rates because tabulation of the records is exceedingly tedious and requires considerable time, whereas the counter automatically summates a subject's separate responses. However, one is not justified in weighing time and effort against scientific accuracy. The kymographic method not only indicates the number of taps made by a subject within a given period of time, but it also indicates the rate of acceleration or deceleration and the accuracy of response in following a given pattern. In the use of counters as recording devices, it is impossible to ascertain the relation between error frequency and the time factor.

While a bank of 4 electromagnetic pens is most desirable for obtaining diagnostic records with the multiple-plate tapping boards, it is possible to obtain satisfactory records with 2 pens only. The hook-up making use of 2 pens is shown in Figure 3. The tapping response of the subject is diagnosed in terms of a given pattern. The alternate plate produces an alternate-stroke pattern, the triple plate produces a "1-2" pattern, while the quadruple plate produces a "2-2" pattern. By means of these patterns it is

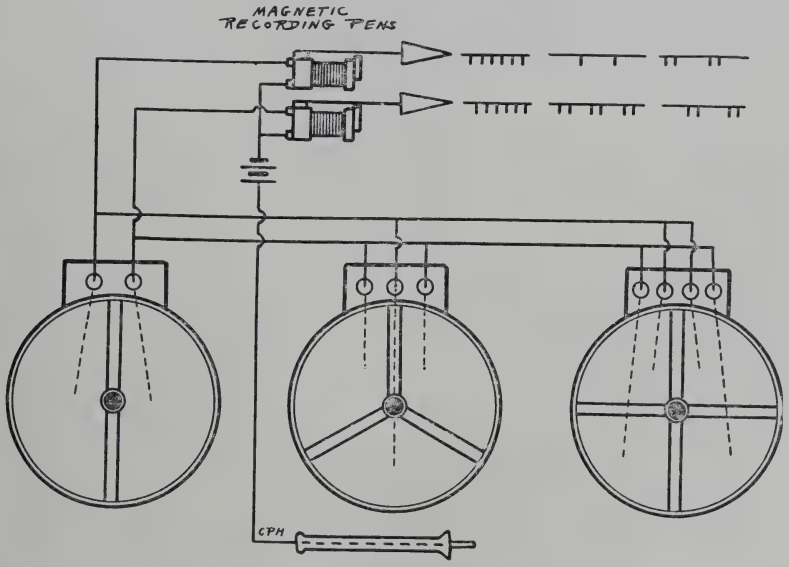

FIGURE 3

ELECTRICAL HOOK-UP SHOWING HOW TWO PENS SUFFICE FOR THREE TAPPING BOARDS

possible to ascertain at what particular point in time a given plate is omitted in response or else repeatedly tapped. The triple- and quadruple-plate tapping boards have proved invaluable in studies of the clockwise and counter-clockwise tapping responses for either hand.

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A SIMPLE MARBLE TIMING DEVICE FOR INVESTIGATION OF PREFERENTIAL MANIPULATION

Christian Paul Heinlein

In an experiment on the nature of preferential manipulation, the writer employed the Dunlap marble trough and marble board for the purpose of ascertaining degree of manual bias in a specific function. In the preliminary stages of experimentation, it was found impossible to time accurately the periodicity of drop or the rate of marble descent by means of simple hand

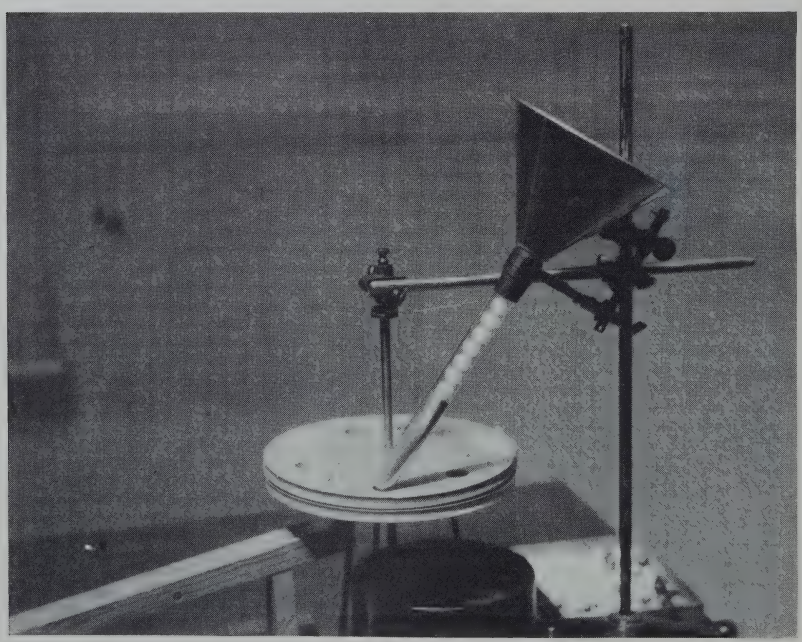

FIGURE 1
Photograph of Assembled Marble Timer

control. Muscular inability to effect simultaneous release of pairs of marbles, slight superfluous finger movements during release, and the formation of perspiration on the skin are a few of the many factors which vitiated against exact stimulus timing.

In order to provide for accurate control of marble release, a mechanical marble dropping device was constructed. This device consists of an automatic marble feed, a timing disc rotated at a wide variety of speeds by means of a kymographic motor, and a stationary release trap. Figure 1 is a photograph of the complete apparatus.

The timing disc is made of sectioned maple, measuring 10 inches in diameter and ½ inch in thickness. The marble carriages consist of 3 pairs of holes, each hole measuring ⅞ of an inch in diameter. The release trap is likewise constructed from maple stock and has the same dimensions as the timing disc. It is supported stationary approximately ⅛ of an inch below the timing disc by means of a metal tripod. A central aperture of 1 inch in diameter permits free passage of the kymograph shaft. The trap hole is 1¼ inches in diameter and is located in line with the circumferential course of the marble carriages.

The marble feed consists of a funnel having a 7-inch bell. The visible feed tube is an ordinary 7-inch glass test tube to which is coupled an aluminum sleeve 4 inches in length and tapered at a 45° angle on the end.

As the marbles are fed through the tube which is supported obliquely at an angle of approximately 45°, they gravitate into the moving carriages of the timing disc and are rolled along the stationary base until they reach the trap hole at which point release automatically takes place. In order to function properly, the marble used should be slightly larger in diameter than the thickness of the timing disc plus the air gap between the timing disc and the stationary base. If the marble used is of such a size that its surface falls below the surface of the timing disc, feed-tube blocking will occur. If the surface of the marble appears above the surface of the timing disc, blocking will not occur since under this condition 2 marbles cannot enter a single carriage.

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AN AUTOMATIC CARD FEEDER AND CATCHER MECHANISM[1]

SIEGEN K. CHOU

In connection with the development of a quadrant tachistoscope (1) for studying reading and legibility of Chinese characters (2) a card feeder and

[1]The cost of constructing this apparatus was partly met by the Thomas Welton Stanford Fund for psychological research.

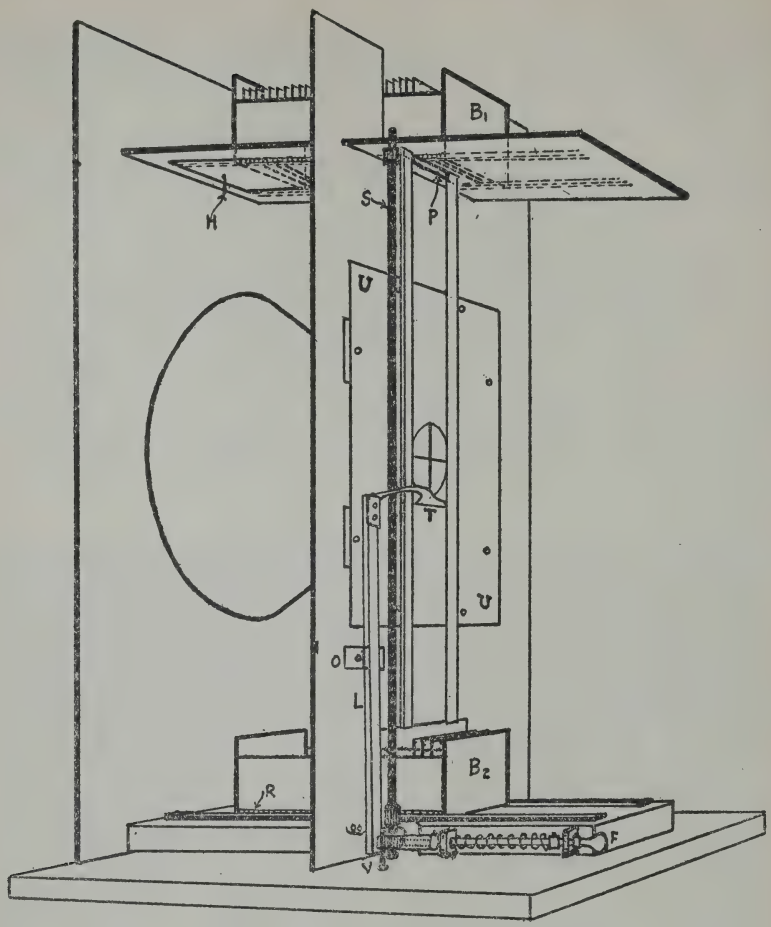

FIGURE 1

SIDE BACK VIEW OF THE QUADRANT TACHISTOSCOPE

Electrical control of the quadrant shutters is omitted in the drawing

catcher mechanism was evolved. This is adaptable for changing and releasing cards in other forms of exposure apparatus. The general principle is that, by a single push of the finger, two duplicate boxes may simultaneously be moved forward one unit distance. Cards stored in the upper box, which is divided by partitions into many thin compartments, drop down to an elastic rest behind the exposure window. Each card after exposure drops into a separate compartment in the lower box.

The two boxes, B_1 and B_2, move on bakelite guiding rails on two platforms. Each box is partitioned by thin aluminum sheets into 10 compartments to the inch and has a capacity of 64 cards. These are all held in the upper box by a sheet-metal bottom that is free to slide back and forth between two grooves on two metal strips screwed to the bottom of the two sides of the box. The bottom is hooked to the platform in *H*. To the side of the left metal strip on the bottom of each box is soldered a piece of rack—*R*, with 10 teeth to the inch, which fits to a pinion on both the top and the bottom of the long brass shaft *S*. Near the lower pinion and on the same shaft is screwed a ratchet of larger diameter. As the farther end of the sliding bottom of the upper box B_1 is hooked on the platform, cards placed in the compartments drop down through the slit *P*, between two guiding strips, to the metal rest *T*, which is attached to the upper end of the lever *L*, pivoted at *O*. At the end of the horizontal rod *F*, an outer flexible brass strip pushes the lower end of the lever *L*, and an inner one (in dotted lines) turns the ratchet on the shaft as the rod is pushed. The outer flexible strip is just a little longer than the inner one so that it causes the lever to detach the card rest from the shutter unit *UU* a trifle before the other strip turns the ratchet to move the boxes. Suppose a card on the rest behind the window has already been read by the subject; pressing the lever will release it immediately to one of the compartments in the lower box. As soon as the card drops clear out of the way of the metal rest, the outer flexible strip that pushed the lower end of the lever slips off on account of the obstruction of the vertical rod *V* over a little ridge on its side and the lever is returned by a coil spring behind it, thus bringing the card rest back in contact with the shutter unit. Meanwhile the inner flexible strip on the rod works the ratchet attached to the shaft, making the entire shaft turn clockwise and thereby causing both boxes to move forward one division. This outward movement of the upper box drops the next card, which in turn is stopped by the card rest and is ready for reading, while the same simultaneous outward movement of the lower box furnishes a new vacant space ready to receive the card as it will drop at the next operation. A simple card catcher bent out of a piece of sheet metal and formed in such a way that the cards may fall on it one above the other can be used instead of the lower box. However, with the box, the cards fall into the same relative compartments as in the upper box. As the upper box is emptied, a third loaded box takes its place; the lower one that has just been loaded by the cards from the upper one is taken away with the bottom locked ready for the next use, and its place is filled by the upper empty box. The box may be designed with its sides and the partitions the same height as the cards, and a groove depression cut across the middle of the entire top to facilitate handling the cards.

The design of the card box was originated from the first crude model using two sets of fairly broad toothed combs fastened to the two sides of the frame of the box for separating the cards. The entire mechanism has the

advantage of such quick automatic control that an experiment involving 128 exposures has been done in as short a time as 25 minutes, while the experimenter had to record the reading reaction time in the tachistoscope with a stop-watch and take it down. The even rate and tempo at which the mechanism works aid the observer in controlling his attention and frees the hands of the experimenter for other duties.

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MURPHY, G. *An Historical Introduction to Modern Psychology.* (Supplement by H. Klüver.) New York: Harcourt, Brace, 1929. Pp. xvii+470.

PILLSBURY, W. B. *The History of Psychology.* New York: Norton, 1929. Pp. 326.

A subject must attain some maturity before it can excite the special kind of interest which produces a history. The appearance of two considerable volumes on the history of psychology in one year and almost simultaneously seems to suggest that in psychology matters have reached an interesting crisis: the new Encyclopedia Britannica, a good index of current opinions, announces a separate article on the historical aspect of psychology and thereby adds another epitaph to the collection. By way of introducing the two important books named above, I shall (at the editor's suggestion) indulge in some generalities on the subject and request the reader to excuse the personal references which will make the task easier and will eliminate a large amount of stilted circumlocution.

In 1908 I became interested in the question where, when, and how did psychology emerge from the undifferentiated mass of literature on human nature and social behavior. As an Oxford student, I had heard Professor Case lecture on Aristotle's psychology and learned something about sensation, memory, habit, and intellect. This was mental philosophy, and it divided the honors with moral philosophy: the latter seemed to have only a feeble connection with the workings of the organism, though the dim religious light of ethics was occasionally refracted by doctrines of emotions and will, never (in those days) by sex or complexes! As a pastime (not prescribed) I read James and began to wonder whether the course of philosophical thought could not be studied with more regard to the expansion of the idea of mind and body. All through the ages, philosophy and science seemed to interact and to have a vital connection at a point which was somewhere between logic and physiology. This clue, once discovered, led to a new reading of

the masters of philosophy and a growing belief that many of them had done valuable work which the prestige of theology and metaphysics had too long obscured. Meanwhile other interests continually intruded themselves. Rhetoric, once a standard topic, was based on the art of controlling juries and audiences, a proper field for practical psychology; education was, on occasion, reformed by some unconventional student of the real child and the actual interests of the young; ethics and politics were occasionally rocked by some irreverent declaration that man is ruled by desires and not reasons; even theology was disturbed by discovering that it started with the dogma that the heart of man is wicked and deceitful above all things. In short, the field was infinite, and the problem of finding justifiable limits was insoluble. In the end, large parts of the manuscript were suppressed and the remainder appeared in three volumes. The comments of the reviewers were interesting. One pronounced the whole work useless, owing to the fitful interest of psychologists in matters of history. Another was puzzled to find it contained so much that was not psychology; he could have earned an honest penny by telling us exactly what is and is not psychology, and the job is still available! In fairness to all concerned, I must mention that commendation was not lacking; the effort was generously appreciated by classical scholars who were not blind to the merits of the Greek psychologists (men like Beare and Shorey), and by modernists of the post-Wundtian School who had culture enough to realize that psychology was not suddenly invented but, like Narcissus, discovered itself in the mirror of human life.

These biographical trivialities are not insignificant. Speaking with a conscious prejudice and claiming no impossible detachment, I think the recent tendency to isolate psychology and breed a curious antagonism toward other aspects of life and thought is an error which must be lived down. More especially, the curious bitterness of the references to philosophy must finally devour itself by being taken into the list of complexes. For who are these people who fill the pages of the history of psychology? Why cannot the historian make his books by contemptuously omitting Plato, Aristotle, Descartes, Hobbes, Rousseau, Lotze, and even James? To the disinterested spectator, of blessed memory, it is peculiarly refreshing to take up a book on that forgotten Greek, Theophrastus, and find that his acute treatment of the senses owes its revival to an American professor of psychology, namely Professor G. M. Stratton. Perhaps, after all, the outcrop of histories is a sign that psychologists are willing to pause a moment and ask themselves where they stand in the great procession of the humanities. Now we have only to wait for the super-historian who will render the last tribute to psychology by writing a history of the historians of psychology!

The backward glance is not without its reward. As a very young student I cherished a copy of Blakey's *"History of the Philosophy of Mind"* (1848). I still have that copy and it recalls the days when I thought it was both good and learned; it now seems incredibly thin and poor. As such it is a

landmark. So far as concerns books in English, it was a gallant effort not to be surpassed for some years. It was succeeded by Bain's sketchy outline, *"Body and Mind,"* which appeared in the old series of the International Library of Science. Sir William Hamilton's edition of Reid's works (1846) was contemporary with Blakey; the famous Note D** was and still is a classic memoir, but it is more a contribution to philology than psychology. The Anglo-Saxon peoples have not been leaders in the history of the sciences or in the history of philosophical thought. Even now we live very largely on translations. In philosophy we have no equal to match Kuno Fischer, Ueberweg, Windelband, or Falckenberg. In psychology the first real attempt was a history of the English achievements written by a Frenchman, namely Ribot's *"Psychologie anglaise contemporaine"* (1870); to which may be added the same writer's history of German psychology. The ancient schools began to receive their due in the sterling work of Siebeck (*"Geschichte der Psychologie"*) and Chaignet, *"Histoire de la psychologie des Grecs"* (Paris 1887-1893). Rohde's *"Psyche"* deserves mention but is nearer anthropology than psychology. More minute and specific work was done by J. I. Beare in his unique study of *"Greek Theories of Elementary Cognition"* (1914). With this date we come to the last 20 years in which the harvest has been more abundant. The modern period naturally attracted attention first. Dessoir's history of modern German psychology was unusually full and accurate for the eighteenth century, from Leibniz onward; but it is relatively a small and sketchy effort. Klemm's *"Geschichte der Psychologie"* (1911, translated in 1914) is distinguished from others by being an effort to present mainly the experimental side. As such it has won deserved recognition and is especially useful for orienting the student in different fields of psychological research, but it does not attempt the wider outlook which treats psychological literature as a whole. A book intended to supply this outlook was Baldwin's *"History of Psychology"* (1915); it proved that anyone who proposed to cover 26 centuries of thought would have to work on a larger scale and with more sympathy for the varieties of human interests. Warren's *"History of the Association Psychology"* should be mentioned as an outstanding contribution to a special part of the whole subject, setting a good example of careful narrative and instructive criticism. To give a complete account of the histories of psychology would require too much space; many other names must be omitted, but these remarks may be taken to indicate what has been done and what peculiar problems arise. For it is necessary to remember that these histories hardly attempt to go beyond general psychology; social, political, educational and applied psychology get sometimes scanty recognition, usually none; while literary psychology, psychology of religion, the writings of mystics and other unprofessional contributors are mostly left in outer darkness.

The works of Murphy and Pillsbury furnish an interesting text for the

epilogue. Though considerably the longer of the two, Murphy's history begins with the modern period, with a preliminary canter through the three centuries before the so-called revival of learning. Columbia University has to its credit two books on modern thought, Burtt's *"Metaphysical Foundations of Modern Physical Science"* and Randall's *"Making of the Modern Mind."* Murphy's book suggests that all these writers have breathed a common atmosphere, with good results. The attempt to keep the psychology in some sort of connection with the civilization of the age under discussion is very commendable; it is a distinct mark of the right attitude for the historian. But considerations of space are apt to disturb the most generous minds, and the result in this case is no more than an occasional gesture, a wave of the hand toward Burtt or Randall. Necessity has made our author very economical. Francis Bacon, for example, comes in (p. 5) solely to be blamed for not living up to the later erroneous opinions about induction; it seems unfortunate that his quite original schemes for a general study of human behavior could not be recognized, more especially as the English empiricists and the French encyclopaedists were inspired by them. The road from Thomas Aquinas to Kant is a little too deeply furrowed; even with the best intentions the mind of teacher or student gravitates into the ruts worn by the funeral processions of Locke, Berkeley, and Hume. To give Bacon his due, one must think more of Comenius or Sir William Petty, of educational, economic or social matters, and of the great declaration that the human mind is not a dry light but glows with suffused emotions.

It would not be fair to the author to concentrate attention on the treatment of the Pre-Experimental Period in Part I. The fact is that the inevitable came to pass. Having rightly decided to construct an approach to modern psychology, the author obviously went on discovering that the approach was more indefinite and more complex than it promised to be. Descartes looks like a good place to begin; but in fact he is an appendix to mediaeval thought and quite unintelligible without some ideas about Roger Bacon, Alhazen, and the history of optics. Hobbes is another favorite place to start; but the fact that his psychological dicta are mostly transcribed from Aristotle's *"Rhetoric"* (as mentioned by Murphy, p. 14) makes the boundary line a little too shadowy. Here we may remark that historians are too much inclined to be distracted by red herrings; as a touch of local color we are told that "Charles I was executed in 1649 and Hobbes published his Leviathan in 1651." As the execution was not a scientific experiment, the reference seems wholly superfluous. It may have had something to do with politics and with the movements of Hobbes, but that it changed one fraction of the views of Hobbes on sensation, memory, imagination, or reasoning cannot be suspected, much less proved. Overworking the environment is a fad that need not be cherished. If Descartes and Hobbes are inescapable landmarks, there is also Malebranche to consider. Unfortunately, he was not considered and only emerged in a footnote on page 217,

where the "James-Lange Theory" elicits the comment that "Lange's theory was in large part derived from Malebranche." Thereby hangs a tale: we read James but not so often Lange's original monograph, and why should Malebranche jut out exactly there? The answer is in the texts, but it is usually assumed that a book written in 1674 by a Roman Catholic "Father" is not likely to be profitable; which is not necessarily true.

The English and French schools of the eighteenth century are described with as much success as could be expected. To call Berkeley an "English ecclesiastic" is perhaps less dangerous in New York than in Dublin, though the Irish in America are not negligible! It is rather curious that our psychologist has here emphasized the philosophy and made Berkeley's "subjective idealism" too conspicuous; he would have been better advised to bring in Malebranche and mathematics, leaving the philosophical deductions to take care of themselves. Hartley and the French school, the Scottish movement (Reid, Brown), and Herbart are dealt with in this part, which concludes with the second great scientific era, the days of Lamarck, Buffon, Cuvier, Bell, and others. Gall is treated with more respect than usual, but the fact that he was a pre-eminent anatomist is obscured by references to a phrenology made popular by Spurzheim and Combe, both impostors. Whether Charles Bell really got his views from Gall is a point I have raised before, but the evidence is difficult to define.

The second part of the book is entitled "From Weber's Experiments to the Age of Wundt." We are still in the field of general history, with the Utilitarians (Mills), Hamilton, and evolutionism to be included. But the German school now occupies the chief place, and the treatment of this period is more exact and more satisfactory than that of the earlier period. The strong point of Murphy's book is the effort to explain the progress of experimental work, and he manages these topics with the assurance of a person well acquainted with current laboratory procedure. In the age of transition there are many loose threads which need to be woven into the fabric in order to anticipate later work. Darwinism is an obvious case, though in relation to the social sciences and culture, it is doubtful whether this "ism" can be made very definite. It is admitted that "evolutionism had been one of the outstanding features of the thought of the last half of the eighteenth century and of the first half of the nineteenth" (p. 116). However, the age of Darwin is a good chronological label; it is anticipated to some degree by the work of Spencer who figures first in the chapter on British Psychology. To estimate Spencer's work is very difficult: at times he seems merely verbose and second-hand; at other times, the principle of synthesis seems to triumph over the complexity of nature and provide a new illumination. Though Murphy does justice to Spencer's evolutionism, he does not seem to give the synthetic philosopher his due merit. To say that "Spencer's psychology never attained the popularity enjoyed by Bain's" is to speak the language of oracles. Bain had authority, influence, and academic

prestige, but the epoch of "best-sellers" had not then dawned. Spencer was widely condemned and very much neglected (Murphy might have noted that the money to produce the synthetic philosophy was collected in America!), but he penetrated and percolated irresistibly. He was stoned by the scientific and the religious critics equally but, like Mr. Bernard Shaw, he realized that "every hit is a boost!"

More light is thrown on what is or is not evolutionism by consideration of the work of Galton. The methods and results of Galton's researches are very characteristic of the period (the age of Darwin) and of the British empirical tradition. Galton devised a lot of beginnings; some of them, like the experiments on association, were overlappings of old and new, others were due to cross-currents of interest in mathematics and anthropology. But indisputably Galton was a "seminal mind" and as such deserves proportionate notice in a historical work. He has been generously treated by Murphy so far as concerns his experimental devices and his method in the study of heredity. His other work, on animal behavior for example, should have been kept in the same context and not left for incidental mention elsewhere; and here we may remark that George Henry Lewes does not get his due, nor does Spencer's plea for comparative psychology get as much emphasis as it deserves. Of course evolutionism made the whole question of animal behavior much more exciting, but Huxley's "automatism" consciously derived its meaning from Descartes and after La Mettrie's *"Man and the Machine"* nothing very new has been said down to Rignano, Needham, and other writers of the last 12 months. The new note struck by evolutionism had a different significance. It was a signal for the movement to denature man (if I may adopt Professor Jastrow's recent words) rather than a new movement toward the acquisition of knowledge about animals. The field-naturalists, the hunters and trappers, had rarely been men who touched the sphere of literary controversy or obtruded themselves on philosophers and theologians. Darwin quotes an astounding number of good observers who probably never before aspired to be considered scientific. So far as the "scientific" study of animals is concerned, the age of Darwin might as well have occurred before the flood. The honors in this field will go to America and the twentieth century; the intensive cultivation of animal behavior in the laboratory has given us a crop of records which have been useful in many ways and may be useful in still more. But, like the canned food industry, the psychology of the non-natural life of animals has its limitations and leads to theories quite as prejudiced as the pre-scientific narratives. We shall have to decide when an animal is most truly an animal before the merits of the two methods can be assessed or Thorndike and Köhler can be finally estimated at their true value.[1]

Contemporary with the evolutionists, we have the great forward move-

[1]See, for example, *J. Gen. Psychol.*, 1929, 2, 115-129.

ment of the physical sciences and their methods. The fashions in psychology have always reflected the broader movements of culture, and the middle of the nineteenth century saw a fairly even division between evolutionism and physics as the directing forces. Helmholtz made his unique contribution by reason of his extraordinary ability to combine mathematics, physics, and physiology into one powerful instrument of research. About Wundt we need say nothing here; naturally he fills a large place and almost (though not quite) fills a long chapter by himself. This is the gateway to the third part and "Contemporary Psychology." Those who have faced the task of making a survey of modern psychology know the oppressive weight of the material available since 1890. The comparative simplicity of the earlier decades with their high lights now vanishes in bewildering confusion. Selection becomes difficult, for "when one considers that the *Psychological Index* carries thousands of titles annually, one may well ask by what right a mere handful of these are mentioned" (Murphy p. xvi). That is the hard lot of the historian who finds his art very long and his life short. Murphy has done well, and only a novice who did not appreciate the difficulties would quibble over the selection or the argument. A few individuals, especially Ebbinghaus and James, stand out as obvious landmarks. For the rest, the subdivisions of modern psychology provide the most effective lines of division, and certain special topics constitute separate fields of work. In general, modern psychology presents new forms of old antithetic doctrines. Sense and reason, association and apperception, mechanism and evolutionism are the words that indicate the most obvious groups. Structuralism and functionalism largely acquired their significance as late forms of the opposition between the ideals of laboratory physics and biological naturalism. The sharper lines of the controversy were blurred by more insistence on the whole personality and an "organismic" standpoint. This might be approached either from the physiological mechanism or the cumulative psychic life. The former method expanded into the behaviorism of the nerve-muscle-gland school. The latter has had a more varied career. It emerged in the Würzburg school as an opposition to the atomistic tendency of some experimental workers; it became a definite standpoint with the exponents of individual psychology and self-psychology; it has allied itself with "mnemic psychology" and with the Freud-Jung-Adler school, because—with all their differences—these psychoanalysts stand for the historical continuity of personal development and for something more specifically psychic than neural modifications. But the historian finds his geographical atlas about as stable as a moving picture. Behaviorism, supported by conditioned reflexes and expanded to mean the cumulative character of experience (thereby taking in memory, mnemic traces, and individual character), seems capable of boxing the compass and leaving nothing to be desired. On page 262 one may read "Hollingworth uses the term 'redintegration' to describe not the process by which an element *brings back*

its context, but the process by which it *functions for* the situation of which it was once a part." We are told that under appropriate conditions Justice and Mercy will meet together and kiss each other. Hollingworth and Sir William Hamilton might presumably stage a similar stimulus-response situation, with the goddess of Subtilty to preside. This is very typical of the fun any historian can get out of the irrepressible tendency of old theories to reappear in new clothes.

The twenty-first chapter of Murphy's work is given to "Measurement of Intelligence." His final chapter and summary inclines to leave the last word with the quantitative methods. This, I should say, was justified so far as history is concerned by the tendencies in America. But the geographical limitation is rather sharply forced upon us by the supplement. This is contributed by Klüver and deals with "Contemporary German Psychology." It opens with a counter-blast in the form of a quotation from Hans Henning to the effect that "until the turn of the century it was believed that one could grasp the mind with number and measure; since 1900 there has developed a qualitative psychology which concerns itself less with numbers and more with kinds of experience and qualitative analysis." If this is sufficiently accurate, the wheel has gone a long way round since Weber and Fechner and Ebbinghaus filled the picture. We will not argue the point, believing that Klüver may be taken as a good judge of the main tendency, though his account is not an inclusive survey. Certainly the atmosphere changes very distinctly as we pass from Murphy's part of the history to Klüver's supplement, and the earlier remarks about psychology and philosophy can be appropriately recalled as we read Klüver's declaration that the German psychologists "up to the present day resort to philosophy." It would have been interesting to have another supplement on French psychology and to have discovered whether these symptoms were developing in that country. Murphy has not neglected the French, but his plan did not include more than passing references to Bergson, Durkheim, Lévy Bruhl, and others, while still more recent trends remained unnoticed. In Germany the class of "pure" psychologists is large and formidable (list page 420) and is strongly flanked by an intermediate group led by Külpe, Marbe, Bühler, and others. Moreover Klüver starts with a cloud of witnesses probably unknown to most American psychologists, Neo-Kantians and the like, but some consolation will be derived from the admission (page 422) that "other trends are not to be overlooked." A different approach might "lead us to the conclusion that the development of 'psychotechnics' (industrial psychology and related fields) is the most conspicuous phenomenon in German psychology." The complete omission of this material from the rest of Klüver's survey seems to be an unfortunate choice of tactics. The account given of personalistic psychology (W. Stern), of the Gestalt movement, and of the more or less comprehensible phenomenology of Husserl is a very valuable contribution to the whole subject; it is probably

more needed than any other and less likely to be found in current treatments of psychology. But while these things are received with proper thankfulness, it seems obvious that the plan of this historical introduction would have been more faithfully preserved if the account of German psychology had been less one-sided and had given a general review of philosophical, technical, educational, and other subdivisions of the work as it is actually being done in that country.

Professor Pillsbury's book illustrates our general remarks about historical method from another angle. Unlike Murphy, who aims to be more emphatically modern, Pillsbury undertakes to present the whole picture "from the Greeks to Darwin"—and afterward. The preface recognizes that, until modern times, the personalities are "already familiar from histories of philosophy," and it is on this account that "the earlier periods are given a very sketchy treatment." Certainly 51 pages cannot be regarded as excessive space for the whole movement, Greek, Hebrew, Hellenistic, and Mediaeval, down to Descartes. What Professor Pillsbury says is, doubtless, quite true, but it is doubtful whether he has said enough to make the statements significant. The plan of the book has led him to give honorable mention to a number of people to whom he cannot devote enough space to make their importance intelligible. The Stoics, for example, are settled in 16 lines, and come rather awkwardly before Hippocrates; the period from Augustine to Thomas Aquinas gets about three pages with the very just conclusion that the latter's contribution makes no advance on previous writers. It is rather uncertain why Geulincx gets a whole paragraph to himself. Malebranche receives more attention than usual, though his connection with the Lange-James theory of emotions is not noticed. The policy of treating the earlier writers sketchily leads to unfortunate results in the case of Hobbes, for we get no hint that his psychology was practically a summary of Aristotle's *"Rhetoric"* or that "decaying sense" is simply a translation of Aristotle's words. So the "father of empirical psychology" scores another triumph, due more to his style than his matter.

The first half of Professor Pillsbury's book produces the general impression that our intellectual ancestors are, historically speaking, a necessary evil. The interest really begins with the nineteenth century, and the author evidently warms to his subject as it proceeds. From Johannes Müller onwards we get more detail with more exact definition of the main points of interest. The position which Professor Pillsbury holds among psychologists gives particular significance and value to the judgments which he passes, implicitly and explicitly, on modern movements and on men who are his contemporaries. He describes the modern schools under six headings (page 271) and the classification used is instructive. One of the best chapters in the book is the account of behaviorism, treated both sympathetically and critically. The account of the Gestalt school should be compared with Klüver's version, and the recognition of a separate "hormic school" is a

convenient way of placing the kind of work most conspicuously done by McDougall.

It is easy to think of many active psychologists who could claim a place in a historical work with as much right as some of those mentioned. But the reader may as well reconcile himself at once to a selection which is dependent on the author's judgment. The aim which Professor Pillsbury set before himself was to provide a readable account of the development of psychology. With more than the average interest in general problems and philosophical movements, he has outlined the origin and development of psychological theory. He appears to have aimed chiefly at simplicity of statement and the elusive factor called human interest. In most cases a biographical sketch introduces the important writer, a very useful feature in a book likely to be read by students who are not acquainted with those individuals in any other connection. Illustrations also enliven the course of events, though the utility of the group facing page 172 is reduced by having no key, and that facing page 235 involves a guessing competition which may flatter some at the expense of others. As it supplies an introduction to the whole range of the subject in a relatively small compass, this book will probably satisfy the present demand for a text-book through which a student can begin to acquire an historical perspective for his work.

Before parting with the two books for which we have to thank these authors, it will be in order to comment on a few details. It is rather peculiar that neither book makes much pretence of providing a bibliography. Murphy is content with eight items, gives the authors no initials, and makes a selection which includes only one general history. This procedure is defended by the statement that the real bibliography is to be found in the text or footnotes. Is the new school giving up the elaborate lists which have bewildered us in most modern books? We hope so, but we do not want them to be reduced to nonentity. Pillsbury improves on Murphy in this matter; he provides select references for each chapter, but he also economizes on initials and makes no distinction between primary and secondary sources. The references for Chapter 2, for example, are Pagel, St. Augustine, and St. Thomas. Siebeck seems to be undeservedly neglected, as he did more for the subject than Burnet, Jowett, and Gomperz put together. In the modern period it is difficult to find reasons for the selection, and in any case *Ravaison* should be *Ravaisson* (page 319) and *Külver* should be *Klüver* (page 320). On the whole the typographical work in both books is very good. On page 200 in Pillsbury the word *extended* seems to be an error; on page 202 Galton's *"Inquiries on Human Faculty"* should be *"Inquiries into Human Faculty,"* etc.; on page 205 *effective* should be *affective;* and on page 156 the name Hall is ambiguous and should be, as it is in the index, Marshall Hall.

Murphy's references are more numerous and often more difficult to print correctly, so that the high level of accuracy deserves praise. The policy

of putting suggestive quotations at the head of a chapter has been followed spasmodically; so far as they are relevant they deserve more exact references. Hippocrates, Plato, Aristotle, or Gregory of Nyssa, to quote some examples, are large words to use without any close definition of chapter and verse. There is also a kind of familiarity in the use of names without initials which presupposes that everyone will know what is meant, a dangerous kind of presupposition. For example Bernard (page 339) should be L. L. Bernard, and in several other cases the task of verifying references would be made easier by giving the names more completely.

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